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TIFIC QUARTERLY DEALING WITH EXTRASENSORY PERCEPTION, THE PSYCHOKINETIC EFFECT, AND RELATED TOPICS

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A FURTHER INVESTIGATION OF TEACHER-PUPIL ATTITUDES AND CLAIRVOYANCE TEST RESULTS

By MARGARET ANDERSON AND RHEA WHITE[1]

ABSTRACT: The third in a series of clairvoyance experiments was carried out with pupils in public school classes. One objective of the series was to discover whether the liking of the pupil for the teacher and the attitude of the teacher toward the pupil have any effect on the ESP scoring rate of the pupil. An affirmative answer was obtained in the first experiment, and this finding has been confirmed in the second and now in this, the third, report. The investigations are concerned also with the more advanced question of the nature of the attitude factors affecting ESP in the classroom.

As in the first experiment in the series, the tests reported here were carried out with high school students (fifth- and sixth-graders were used in the second experiment), and the general testing procedure was the same. The teacher administered the tests. Each subject was given a sealed opaque envelope, prepared at the Parapsychology Laboratory, containing a list of randomized ESP symbols, and he was asked to write, on a record sheet stapled to the outside, what he thought the symbols were. He also filled out a confidential questionnaire designed to reveal his liking for the teacher. The teacher, likewise, was asked to record his attitude toward each pupil. All the records, unopened, were returned by mail to the Laboratory for checking.

There were 205 subjects in the third experiment, and a total of 25,625 trials. The general scoring trends were the same as in the two previous experiments. The main interest of the investigators was not in the total results, but rather in the group scores when the data were separated into the various attitude categories. It was found that the pupils whom the teacher liked gave significantly positive scores, while those he disliked gave insignificantly negative scores. There was a significant difference between the two groups. When the results were divided according to whether the student was highly positive, moderately positive, or negative in his attitude toward the teacher, the first

[1] The authors were supported in the work reported here by grants from the Parapsychology Foundation, Inc., of New York City, and both hold Ralph Drake Perry Fellowships in the Parapsychology Laboratory of Duke University. They wish to express their appreciation for the cooperation given by the seven teachers and their students.

group gave insignificantly positive scores; the second, significantly positive scores; and the third, insignificantly negative scores. The difference between the total ESP results of the combined groups with a favorable attitude and the ESP results of the group with a negative attitude was significant. The highest ESP score average was made by the selected group of pupils who liked their teacher and whom the teacher liked; that is, the mutually-favorable-attitude group. At the other extreme, the lowest ESP average was made by the mutually-unfavorable-attitude group.

In this report, the data of all three teacher-pupil series (involving 546 subjects and 65,275 trials) are compared and combined. From this combination there emerges evidence of a high order of lawfulness in the relationship between ESP scoring of the pupil and the attitudes registered by teachers and pupils toward each other in the classroom situation.—Ed.

Introduction

In an earlier paper (2), we reported an experimental study of the relation between ESP test performance by high school students and the attitudes (liking or disliking) of the students and their teachers toward each other. It was part of a larger inquiry into the conditions affecting the functioning of psi capacity, with special concern for the subtle interpersonal influences that may help or hinder psi.

In a review of the relevant literature in that article, we drew attention to specific attempts to pair up, as subjects and agents in GESP tests, persons who liked each other, were closely related, were engaged or married, etc. A few attempts have also been made to rate the attitudes of subject and experimenter toward each other and to correlate them with the level of ESP scoring. Although these experiments were undertaken with the expectation that high scoring would be associated with warm, friendly, and favorable attitudes, this was not always the case. However, in most of the experiments, not enough runs were done to yield satisfactory conclusions. One might question whether the conflicting results of these earlier tests may not have been due to the experimenters who, while taking the interpersonal relationship into account, did not conduct their tests in situations where the relationship might have been functioning as an everyday occurrence.

Unlike earlier experimenters, J. G. van Busschbach (3, 4, 5) conducted his tests in a classroom where interpersonal relations were already established and operating prior to the ESP test.

Using a GESP test on fifth- and sixth-grade children in Holland and in America, he obtained highly significant results when the teacher served as "sender," but the same test gave insignificant results when the sender was an adult other than the regular teacher or was a pupil in the class. These tests were conducted and administered by test leaders who were not the children's teachers. From this work it becomes clear that when the test was inserted into the classroom situation, significantly positive ESP scoring occurred. No effort was made to determine the nature of the teacher-pupil relationship, and it is possible that negative attitudes may have been present which diluted the over-all positive deviation. Also, further research is needed to determine the role the test leader may play in the operation of ESP in the van Busschbach tests.

In the first Anderson-White experiment (A-W 1), the authors, with the cooperation of seven high school teachers, carried out clairvoyance tests on students in seven academic classes. The teacher administered the test. The objective was to discover whether any effect on scoring rate in the test might be associated with the attitude of the teacher toward the individual pupil or the liking of the pupil for the teacher, but primarily the combination of the two. There were 28,500 trials, carried out by 228 subjects. Teacher and pupils filled out questionnaires designed to reveal their attitudes toward each other.

It was found that the students preferred by the teacher scored above the chance mean in the ESP tests while those not preferred scored below the mean. The difference was statistically significant and was consistent. When the total results were divided according to whether the student was highly positive, moderately positive, or negative in his attitude toward the teacher, the first group gave significant positive results; the second, chance results; and the third, significant negative results.

When the teacher and pupil attitudes were combined, it was found that along with positive attitudes on the part of the teacher and pupil toward each other went ESP results with a highly significant positive deviation. On the other hand, the combination of negative attitudes gave results with a significant negative deviation. In addition, a chi-square test of consistency applied to the

combination of attitudes on the part of the teacher and pupil gave a highly significant result.

In November, 1956, an opportunity was provided for one of the authors to extend the teacher-pupil attitudes and clairvoyance test research to the fifth and sixth grades (1). Four classes were tested in the same school in a suburb of Chicago. The test was similar to A-W 1 except that fewer trials were made and the student questionnaire was adapted to fit the fifth-sixth grade level.

This series consisted of 446 runs of ESP trials by 113 subjects, and the data were analyzed, as in the previous study, in relation to teacher and student attitudes. The teacher's attitude toward the students was related to the ESP scoring rate in the same direction as in the A-W 1 experiment and with approximately the same difference in average scores. With the smaller number of runs in this second experiment, this difference, unlike A-W 1, was not significant, but the pupils the teacher liked gave an average above the mean chance expectation and those whom the teacher disliked averaged below mean chance expectation. As in the earlier study, the student's attitude toward the teacher divided the ESP scores to a significant extent, with those who liked the teacher averaging above chance and those who disliked him averaging below. When the two attitudes were combined, it was found that the scores of the subjects who liked and were liked by the teacher were positive, and those of the students who disliked and were disliked by the teacher were negative. The difference between the two opposing groups was significant, with $P=.0006$. It should be emphasized that this second experiment yielded the same type of relations to a significant degree with only half as many subjects as the first.

The purpose of the present experiment, conducted in November, 1956, was to extend further the investigation of the relationship between teacher-pupil attitudes and clairvoyance test results in the high school classroom indicated by A-W 1. The only change between this experiment and A-W 1 was the introduction of seven different high school teachers of academic subjects (English, mathematics, history, psychology, chemistry, and biology) and in six different states.

Procedure

Seven academic high school teachers were asked to participate in this experiment. Six of them were known directly or indirectly by one or both experimenters. The seventh one was recommended by a friend of M.A. When the request was made, the teachers were told that they would have to: (1) distribute the student test envelopes; (2) allow their students to answer a questionnaire concerning the teacher (a copy of the questionnaire was sent to the teacher at this time); and (3) indicate their attitude toward each student participating in the test. All seven teachers promptly replied that they would be willing to cooperate.

The teachers were asked to select a class of their choice for the experiment. Each teacher received a standard sheet of instructions (see A-W 1) with which he was asked to familiarize himself before administering the test. When the test was given, the teachers were asked to "tell," not "read," it to the class. As in A-W 1, no reward was offered. The teacher expressed his request and desire that the student do his best both for the student's own self, but even more, for the teacher.

At the beginning of the test, each student received an envelope containing a standard ESP record sheet on which were written five runs of ESP symbols, twenty-five trials to each run. These target sheets had been made up at the Parapsychology Laboratory from *Tables of Random Sampling Numbers* by M. G. Kendall and B. Babington Smith, 1954. A different target order was used for each sheet; the sheets were numbered, and a carbon copy of each was kept on file at the Parapsychology Laboratory in R.W.'s possession. Each target sheet was placed between three sheets of opaque paper—two on top and one underneath—and enclosed in a manila envelope. Two blank ESP record sheets for the subject's calls were then stapled to the outside of every envelope, serving as the means also of sealing the envelope. There was a piece of carbon paper between these two outside sheets so that a copy of each subject's ESP calls could be obtained. The carbon copies were used for an independent check.

Each student was asked to write on the topmost outer record sheet the symbols that he thought were written on the target

sheet enclosed within the envelope. The students were allowed to make their calls in their own way and at their own rate of speed.

A small white envelope was stapled on the back of the manila test envelope. It contained a questionnaire regarding the student's opinion of the teacher. (See Appendix in A-W 1.) After the ESP test was completed, the teacher asked the students to answer the questionnaires and then to replace them in the white envelopes and seal them. He emphasized that the students should answer honestly and he assured them that he would not see their answers.

Finally, the teacher completed a statement of his own regarding his acceptance or rejection of each individual student in his class. The question, to which a *yes* or *no* answer was requested, was: "If you could form your ideal group for this class, would you include this student?" It was thought that a question stated in this manner, although it did not directly ask whether or not the teacher liked the student, would nevertheless get at this fact indirectly. The teacher, it was assumed, would probably have to consider all of the various factors making up his attitude toward the student in order to answer this question.

When the tests had been completed, the teacher mailed all of the material, unopened, to M.A. at the Parapsychology Laboratory. The sheet on which the teacher had recorded his attitude toward each student provided an alphabetical listing of the names of all the students in the class. From this sheet, M.A. and R.W. independently transferred the students' names and teacher attitudes so that each had in her possession a master copy of the subjects.

Next the two experimenters jointly removed each questionnaire from the white outside envelope and copied on it the name of the student, which had been written on the topmost outer record sheet. (In order to encourage frank answers, the students had not been requested to put their names on the questionnaires.) The questionnaires were then divided in half, and M.A. in her office totaled the attitude rating for her half of the students, recording the classification on her master list. R.W. took the other half to her office and independently rated and recorded the attitudes in the same manner. The attitude sheets were then exchanged and the last

half of the independent classification and recording by each experimenter was completed. At this point, the two experimenters checked their master sheets to see if there was agreement on the number and names of the students, the ratings on the student attitudes, and the teacher ratings. If a discrepancy was found, each experimenter independently re-examined the original sheet and the two then agreed as to the correct rating.

Next, both authors unsealed and opened the ESP envelopes. The name which each student had written on the outer record sheet was then written on the inner target record sheet and on the carbon copy of the target sheet which had been in R.W.'s keeping at the Laboratory. R.W. checked the calls on the student's original call sheet against the carbon copy of the target sheet, while M.A. checked the carbon copy of the student's call sheet against the original target sheet. These duplicate scores were independently recorded on the master sheet of each experimenter, and they were then checked run-by-run for agreement. Later on, Mrs. Stober of the Parapsychology Laboratory checked the scores call-by-call to insure against the possibility that both experimenters might have overlooked a hit in the same run. In this way, independent tabulations were made on each checker's master sheet for each student on the basis of his ESP score, acceptance or rejection of him by his teacher, and his own classification of the teacher into one of the ten possible attitude scores.

Results

The data were collected by seven secondary school teachers from 205 students (distributed in grades 9-12), who were given a total of 1025 runs or 25,625 trials. In keeping with the purpose and design of the experiment, these results, as in the two previous reports, were analyzed in relation to student and teacher attitudes.

The extent to which ESP scoring is related to the direction of the teacher's feeling toward the student is shown in Table 1. Where the teacher is positive in feeling toward 121 subjects, the average run score is 5.26, and the CR=3.25. The 84 subjects to whom the teacher is negative in feeling give an average run score

Table 1

ESP Results as Related to Teacher's Attitude Toward Student

Teacher's Attitude Toward Student	No. of Subjects	No. of Runs	Av. Run Score	Deviation	CR	P
Positive	121	605	5.26	+160	3.25	.001
Negative	84	420	4.85	− 61	1.49	....
Total	205	1025	5.10	+ 99	1.55	....

CR_d (Pos. attitude—Neg. attitude) = 3.20
P = .001

of 4.85. The CR of the difference between the two groups is 3.20 (P=.001).

The evaluation of the difference between the total trials and hits of two groups by means of the CR based on the binomial hypothesis is, of course, only a statistical test of the chance hypothesis and it is therefore strictly relevant only to the question of evidence for ESP. Some other method for evaluating the difference between groups in a way that makes due allowance for the possible variation introduced by ESP itself must be used if one is to draw any conclusion regarding the effect of the difference in conditions (e.g., positive and negative teacher attitudes) upon the ESP process. In this paper, as in the previous two studies, we have used for this purpose the chi-square test of the proportions of students above and at-or-below mean chance expectation in ESP scores under the two attitude groups in question.

In order to evaluate in this way the consistency of the students' ESP scoring when the teacher was positive or negative in feeling, the results were arranged in a two-by-two contingency table. The number of subjects whose total score was above mean chance expectation (M.C.E.), and the number of subjects whose scores were at or below M.C.E., divided according to the teacher's feelings, are given in Table 2. The results of this analysis of individual scoring trends in relation to teacher attitudes is significant (P=.015).

Another aspect of the experiment dealt with the direction of the student's feeling for the teacher in relation to his rate of scoring in ESP. The 84 students most favorable to the teacher (10-9 level)

Table 2

STUDENT ESP SCORE IN RELATION TO TEACHER'S ATTITUDE TOWARD STUDENT

	Teacher Positive	Teacher Negative	Total
Students with ESP Scores Above M. C. E.	67	32	99
Students with ESP Scores at or Below M. C. E.	54	52	106
Total	121	84	205

$x^2 = 5.93$ (1 d.f.)
P = .015

give an average run score of 5.16. At the other end of the scale, 55 subjects with a negative attitude toward the teacher obtained an average run score of 4.73. The CR of the difference between these two groups is 2.77. The 66 subjects who are in the average-to-positive classification of feeling for the teacher (8-7 level) give an average run score of 5.33, CR=2.97. (See Table 3.)

Table 3

ESP RESULTS FOR THE THREE LEVELS OF STUDENT'S ATTITUDE TOWARD TEACHER

Rating of Teacher by Student	No. of Subjects	No. of Runs	Av. Run Score	Deviation	CR
10-9 (Positive)	84	420	5.16	+ 66	1.61
8-7 (Positive)	66	330	5.33	+108	2.97
Total Positive Ratings	150	750	5.23	+174	3.18
6-0 (Negative)	55	275	4.73	− 75	2.26

CR_d (Total pos. rating—Total neg. rating) = 3.57
P = .0004

Combining all of those who are average-to-positive in feeling, the 10-7 level, 150 subjects give an average run score of 5.23. The CR of the difference between these positive 150 subjects and the 55 negative-reaction subjects is 3.57 with P=.0004.

A chi-square test of consistency was made of the distribution of ESP hits and misses in relation to the three levels of the student's attitude toward the teacher. The chi square of the three-by-two table is 13.98 (2 d.f.) with P=.0009. (See Table 4.)

Table 4

CHI-SQUARE DISTRIBUTION OF ESP SCORES IN RELATION TO STUDENT'S ATTITUDE TOWARD TEACHER

ESP Results	STUDENT'S RATING OF TEACHER			Total
	10 - 9 Level	8 - 7 Level	6 - 0 Level	
Observed Hits	2166	1758	1300	5224
Expected Hits	2141	1682	1402	
Observed Misses	8334	6492	5575	20401
Expected Misses	8359	6568	5473	
Total	10500	8250	6875	25625

$\chi^2 = 13.98$ (2 d.f.)
P = .0009

In order to determine the consistency of the scoring obtained when the student was positive or negative in feeling, the results were arranged in a two-by-two contingency table. (See Table 5.) All subjects whose ratings were positive (both the 10-9 and 8-7 levels) are included in the positive group in the table. The chi square is 5.69 (1 d.f.) with P=.017.

The results of the ESP scoring when the teacher and the student are both favorably disposed to each other are given in Table 6. Sixty-one subjects who rate their teacher 10-9 were also given positive ratings by their teachers. The average run score is 5.28. At the other end of the scale, 35 subjects experiencing a mutually unfavorable relationship with the teacher give an average run score of 4.53. The CR of the difference between these two conditions is 3.96 (P=.00007). Combining the highly positive (10-9 level) and the average-to-positive (8-7 level) students when the teacher is positive gives an average run score of 5.30. The CR of the difference between this group and the mutually negative group is 4.38 (P=.00001).

Here again a two-by-two contingency table was made to ascer-

Table 5

STUDENT ESP SCORE LEVEL IN RELATION TO STUDENT'S ATTITUDE TOWARD TEACHER

	Students with Positive Attitude	Students with Negative Attitude	Total
Students with ESP Scores Above M. C. E.	80	19	99
Students with ESP Scores at or Below M. C. E.	70	36	106
Total	150	55	205

$x^2 = 5.69$ (1 d.f.)
P = .017

Table 6

ESP RESULTS AS RELATED TO ATTITUDE OF TEACHER AND STUDENT TOWARD EACH OTHER

Rating of Teacher by Student	TEACHER'S ATTITUDE POSITIVE					TEACHER'S ATTITUDE NEGATIVE				
	No. of Subjects	No. of Runs	Av. Run Score	Dev.	CR	No. of Subjects	No. of Runs	Av. Run Score	Dev.	CR
10-9 (Positive)	61	305	5.28	+ 86	2.46	23	115	4.83	−20	....
8-7 (Positive)	40	200	5.33	+ 66	2.33	26	130	5.32	+42	1.84
Total Positive Ratings	101	505	5.30	+152	3.38	49	245	5.09	+22	....
6-0 (Negative)	20	100	5.08	+ 8	....	35	175	4.53	−83	3.14

CR_d (Total Pos. rating—Total Neg. rating) = 4.38
P = .00001

tain the consistency of scoring when the teacher and the student were mutually positive and when the teacher and the student were mutually negative. (See Table 7.) The resulting chi square is 9.82 (1 d.f) with P=.002.

Table 7

STUDENT ESP SCORE LEVEL WHEN STUDENT AND TEACHER HAD SAME ATTITUDE TOWARD EACH OTHER

	Teacher and Student Positive	Teacher and Student Negative	Total
Students with ESP Scores Above M. C. E.	57	9	66
Students with ESP Scores at or Below M. C. E.	44	26	70
Total	101	35	136

$x^2 = 9.82$ (1 d.f.)
P = .002

COMPARATIVE RESULTS OF THE THREE STUDIES

The significance of the present report may be better viewed in comparison with the two preceding experiments (**1**, **2**). The three studies (A-W 1, M.A. 1, and A-W 2) involve 14 high school teachers of 9 different academic subjects in 9 states testing 433 students, and two fifth- and two sixth-grade teachers testing 113 students. It should be pointed out that while the high-school students did five runs apiece at one session, the elementary school children did two runs in two sessions for a total of four runs. Among the elementary grade pupils, three children were absent for the second session. This accounts for the seeming discrepancies in the following tables where the number of runs is given as 446 when the total would normally have been 452. The data, therefore, concern 18 teachers and 546 students, the latter of whom were given 2611 runs or 65,275 trials. The total results of the three series are shown in Table 8 for whatever incidental interest they may have for the reader. (See Table 8.)

The next analysis deals with ESP scoring as related to the direction of the teacher's feeling toward the student. Three hundred and thirty-three subjects liked by the teacher give an average run score of 5.21 on 1581 runs, CR=4.20 (P=.00003). Where the

Table 8
TOTAL ESP RESULTS OF THREE STUDIES

Experiment	Number Teachers	Number Subjects	Number Runs	Av. Run Score	CR
A-W 1	7	228	1140	5.07	1.15
M. A. 1	4	113	446	4.99	....
A-W 2	7	205	1025	5.10	1.55
Total	18	546	2611	5.07	1.67

teacher is negative in feeling toward 213 subjects, the average run score is 4.84 on 1030 runs, CR=2.54 (P=.01). The CR of the difference between the positive and the negative groups is 4.63 ($P=4 \times 10^{-6}$) (See Table 9.) In order to evaluate the consistency of the scoring obtained when the teacher was positive or negative in feeling, the results were arranged in a two-by-two contingency table (not shown in the paper). This analysis of individual scoring trends in relation to teacher attitude gives a chi square of 11.01 (1 d.f.) with P=.0009 as shown in Table 9.

Table 9
ESP RESULTS AS RELATED TO TEACHER'S ATTITUDE TOWARD STUDENT

Experiment	Teacher Positive			Teacher Negative			P of CR_d (Pos.-Neg. Ratings)	P of χ^2 Consistency
	No. Runs	Av. Run Score	CR	No. Runs	Av. Run Score	CR		
A-W 1	660	5.22	2.80	480	4.86	1.51	.003	.09
M. A. 1	316	5.09	....	130	4.72	....	.08	.048
A-W 2	605	5.26	3.25	420	4.85	1.49	.001	.015
Total	1581	5.21	4.20	1030	4.84	2.54	4×10^{-6}	.0009

A third aspect of the study deals with the relation of the student's feeling for the teacher to his rate of ESP scoring. The 230 subjects most favorable in feeling toward the teacher (the 10-9 level) give an average run score of 5.26 on 1103 runs, CR=4.26 (P=.00002). The 149 subjects with a negative attitude toward the teacher give an average run score of 4.64 on 708 runs, CR=4.74

($P=2 \times 10^{-6}$). The CR of the difference between these two groups is 6.46 ($P=1 \times 10^{-9}$). (See Table 10.)

Table 10

ESP Results for the Three Levels of Student's Attitude Toward Teacher

Experiment	10 - 9 (Positive)		8 - 7 (Positive)		10 - 7 (Positive)		CR Total Pos. Ratings	6 - 0 (Negative)		CR Total Neg. Ratings	P of CRd (Pos.-Neg. Ratings)	P of χ^2 Consistency
	No. of Runs	Av. Run Score	No. of Runs	Av. Run Score	No. of Runs	Av. Run Score		No. of Runs	Av. Run Score			
A-W 1	515	5.35	330	5.09	845	5.25	3.58	295	4.56	3.78	6×10^{-7}	.0003
M. A. 1	168	5.23	140	4.99	308	5.12	1.05	138	4.66	2.00	.025	.14
A-W 2	420	5.16	330	5.33	750	5.23	3.18	275	4.73	2.26	.0004	.017
Total	1103	5.26	800	5.17	1903	5.22	4.80	708	4.64	4.74	1×10^{-9}	.00001

Combining all those who are average-to-positive in feeling, the 10-7 classification, the 397 subjects give an average run score of 5.22 on 1903 runs, CR=4.80 ($P=2 \times 10^{-6}$). The CR of the difference between this group and the 149 subjects who are negative in feeling toward the teacher is 6.59 ($P=1 \times 10^{-9}$). (See Table 10.)

In order to determine the consistency of the scoring obtained when the student was positive (rating 10-7) or negative in feeling, the results were arranged in a two-by-two contingency table. The chi-square is 20.18 (1 d.f.) with P=.00001. (See Table 10.)

The most important aspect of the three studies concerns the interaction of the teacher's and the student's feeling for each other. The results of the ESP scoring when the direction of the teacher's and student's feeling is mutual are presented in Table 11. The 269 students who share a mutually positive feeling with the teacher give an average run score of 5.31 on 1285 runs, CR=5.54 ($P=4 \times 10^{-8}$). The 85 subjects who experience a mutually negative feeling with the teacher give an average run score of 4.54 on 412 runs, CR=4.66 ($P=3 \times 10^{-6}$). The CR of the difference between the mutually positive and the mutually negative groups is 6.80 ($P=3 \times 10^{-12}$).

A two-by-two contingency table was again made to determine the consistency of scoring when the teacher and student were mutual-

ly positive and mutually negative. The resulting chi square is 23.08 (1 d.f) with P<.00001. (See Table 11.)

Table 11

ESP Results as Related to Attitude of Teacher and Student Toward Each Other

Experiment	Total Mutually Positive			Mutually Negative			P of CR_d (Pos.-Neg. Ratings)	P of χ^2 Consistency
	No. of Runs	Av. Run Score	CR	No. of Runs	Av. Run Score	CR		
A-W 1....	550	5.37	4.39	185	4.63	2.50	$< 1 \times 10^{-4}$	< .005
M. A. 1...	230	5.17	1.29	52	4.27	2.64	.003	.006
A-W 2....	505	5.30	3.38	175	4.53	3.14	.00001	.002
Total...	1285	5.31	5.54	412	4.54	4.66	3×10^{-11}	< .00001

Discussion

The present study may be about as near to a replication of an experiment as the psychological sciences will allow. It is not, however, the purpose of this paper to probe the question of the repeatability of psi experiments. Divergent meanings and interpretations leave the issue confused, and experimental results rather than arm-chair philosophizing will have to put the question in its proper perspective. It does appear, however, that one of the most basic and fundamental aspects regarding an ESP test is frequently overlooked; namely, the psychological factors involved. While this point is so obvious as to seem irrelevant to a discussion, the appreciation of it often appears lacking. While psychological factors exclude the possibility of repetition in the strict sense of the word, they do allow extension to similar situations, and, actually, it is by extension, with a view to finding regularity, that science proceeds. The main contention of the reported research here, showing the outstanding order of consistency that it does, is that it is a successful extension.

The A-W 1 experiment was extended to new participants with no alteration of procedure. Naturally there were differences: there were different teachers, students, degrees of interest, academic subject matter, schools, and geographic location. But as far as pos-

sible the two sets of participants were similar. Accordingly, the importance of the closely similar results is considerably greater in the display of underlying lawful relations in the ESP effects produced.

The three questions involved in this research have been directed at the teacher's attitude toward the student, the student's attitude toward the teacher, and the interaction of these attitudes as related to the direction of ESP scoring. The comparative results indicate an increased sign of consistency from A-W 1 to A-W 2 on the teacher attitude. (See Table 9.) On the other hand, a lower level of consistency occurs from A-W 1 to A-W 2 on the student attitude. (See Table 10.) However, it appears that the student attitude is more significant than that of the teachers. The most important aspect of the research has been the interaction of mutual attitudes, and it is here that the most striking similarities, showing a remarkable order of consistency, are evident. (See Table 11.) The scoring level differences, discernible from the tables, are hardly worth mentioning since they are negligible, even the CR's of the difference, but there are proportionate shifts noticeable in the column showing the consistency of the results which suggest the fluidity of the situation.

An interesting aside came out of the A-W 1 experiment. When the teacher was positive toward the student, but the student's feeling was negative toward the teacher, the 22 subjects gave a negative CR of 2.96. Thus there appeared a similarity in the two conditions of teacher positive, student negative; and teacher negative, student negative. This was puzzling and posed the question of how important the teacher's attitude is if the student is negative in his feeling. In the M.A. project, the 22 corresponding subjects gave an average run score of 4.90 on 86 runs, while in the A-W 2 experiment, the 20 corresponding subjects, doing 100 runs, gave an average run score of 5.08. We can only say that this does not bear out the A-W 1 results on this point and it may, therefore, be taken less seriously.

The most significant aspect of the research is the appearance of a lawful operation evidenced by the high level of consistency in the relationship between ESP scoring and the attitudes registered. It

is apparent from the van Busschbach work and the experiments reported here that ESP does operate in the classroom. It remains for patient research to seek out the variables in the situation which ultimately may lead to an understanding of the lawfulness underlying these phenomena.

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A CONTINUATION OF THE STUDY OF PSYCHOKINESIS AND PHYSICAL CONDITIONS

By H. FORWALD

ABSTRACT: In the experiment reported, Mr. Forwald, using himself as subject, has continued his studies of the psychokinetic force necessary to cause falling cubes to roll toward a specified side of the table. His apparatus, as in former series, consists of an electrically released device, an inclined plane and a horizontal walled-in surface, onto which the objects fall.

In a previous paper Mr. Forwald presented a formula to measure the average side force (in dynes) necessary to cause the observed displacement. In previous studies he found that the greatest effect occurred in the first of the five throws (or releases) in the set. On this first throw of the set a significant displacement occurred in the wished-for direction. The side force measured was about the same for both wooden and bakelite cubes.

The present report confirms earlier results with wood and bakelite cubes, but shows that greater force was indicated by the results when wooden cubes coated with aluminum were used, and still greater force when a heavier coating was used. Pure aluminum gave still higher figures (in dynes) and the highest of all were aluminum cubes with paper-covered faces. Mr. Forwald recognizes the obviously possible role of the subject's preference and expectation for the various types of cubes, but for the present considers the physical differences (surface as well as substance) as probably playing a part in conjunction with the mental factors.—Ed.

INTRODUCTION

IN A PREVIOUS paper (4) a new method was presented for the investigation of the dynamical behavior of a moving cube aggregate when it is subjected to mental influence. With this method the experimental apparatus consists of a horizontal dice table marked off in centimeters lengthwise (X-direction) and crosswise (Y-direction) so that the positions of cubes, released from one end onto the table, can be measured. The cubes enter the table with a certain velocity along the X-axis, and the subject tries to influence them mentally to veer to one side or the other; i.e., he tries to cause an aberration along the Y-axis. The table is divided into two halves longitudinally and an equal number of trials is made for each side. The difference between the aberrations in these two cases is a measure of the physical influence on the cubes due to the mental efforts of the subject. It is possible on the basis of the differential aberration to calculate the mean side force K which it is necessary to introduce in order to account for the observed aberration.

It was shown in the paper that in experimental series with wood cubes a mean side force of about 15 percent of the cube weight would be necessary in order to obtain the observed aberration of the cube aggregate from the movement expected on the basis of established mechanical laws.

The investigations into the dynamics were followed up; and in a recent paper (6), a report was given on experiments with heavier cubes made of bakelite. The results of the experiments with wood and bakelite cubes indicated a relationship between the aberration attributable to the mental influence and certain physical conditions, namely, the cube weight and the roughness of the cube surface.

In the present paper a report is given of further experiments investigating the psychophysical problem. They can be grouped in the following four sections.

Section I comprises experiments with wood cubes, rough bakelite cubes, and aluminum cubes. The cubes of aluminum were introduced here for the first time in these "scaled experiments"; i.e., experiments in which the cube positions are measured in a coördinate system. As concerns the bakelite cubes, the previously found value of the mean side force K was confirmed in the new series. But the side force found necessary to give the effects obtained with aluminum cubes was much greater than what would be expected according to the previous findings.

Therefore, in *Section II*, new and extensive experiments were conducted with aluminum cubes in order to investigate whether the high K-values would repeat themselves. In fact, they did, and the problem concerning the influence of physical conditions on the results therefore had to be reconsidered in certain respects. Indications were found that the surface conditions of the cubes apparently are of greater importance than previously expected.

The new line was followed up in *Section III*, which comprises comparative experiments with wood cubes which in some series had their natural surface but in further series were provided with a coating of aluminum foil. In one case this coating had a thickness of 8 μ (8 microns, or .008 mm.), and in another case a thickness of 150 μ. Substantial differences in the mean side force K were found in these three experiments with wooden cubes.

The results arrived at in the previous sections called for an additional experiment using aluminum cubes with a nonmetallic coating. These experiments, described in *Section IV*, were carried out with aluminum cubes having a paper coating 190 μ in thickness.

Experimental Apparatus and Procedure

The author, as before, was the only subject and he did his own recording.

A detailed description of the apparatus and the procedure is given in the previous paper (4). The following is a summary of the main features.

The horizontal dice table, measuring 80 by 112 cm., is provided with a coördinate system. Ink lines drawn parallel to the table sides for each centimeter are numbered 0 to 112 in the longitudinal direction (X-direction), and 0 to 80 in the transverse direction (Y-direction). At one end of the table, elevated about 30 cm. above the table surface, there is a release mechanism with a V-shaped channel container in which a row of cubes can be placed. Prior to the release, the container is in a horizontal position, but when released (electrically, by means of a push-button at the end of a cord) the container swings down by gravity like a trap door. The cubes then leave the channel, and after a fall in free air they hit a slope which curves out onto the horizontal dice table. Thus they enter the plane with a certain velocity and scatter over the surface. The channel is arranged so as to get the cubes to enter the plane along the line $y = 40$, which is the mid-line of the table in the longitudinal direction. Zero in the X-direction is at the same end of the table as the release mechanism; zero in the Y-direction is at the right as viewed from behind the release mechanism. The side of the table from zero to 40 crosswise is the A-side; from 40 to 80 is the B-side.

The standard experimental unit used in this research is the *set*, which comprises five releases of six cubes with the subject "willing" the dice to move toward the A-side, followed by five releases in which he wills them to move toward the B-side. A standard set thus comprises $5x6x2 = 60$ single die falls, or trials. A *series* is made up of five such sets, totaling $60x5 = 300$ trials, 150 of which have the A-side as target, and 150 the B-side.

After each release, readings are taken of the cube positions on the

plane. The arithmetical mean values of y are calculated for B- and A-targets separately, and if the difference between these two values, $2y'_o$, is positive and sufficiently large, then there is bearing for the assumption that the cubes have followed the experimenter's intention. This can be decided, for instance, on the basis of a Student's t test of the individual readings. If this test leads to a probability figure of $P = .02$ or below, then the result is said to be significant; i.e., it can be considered as indicating the action of some factor not generally recognized under the actual experimental conditions. The Student's t test is a statistical analyzing procedure used in quantitative scientific investigations, and a reference for it is, for instance, *Fisher* (**1**).

It is possible on the basis of the difference $2y'_o$ to calculate the side force K which is necessary to account for the aberration of the cube aggregate. The following formula for K was calculated in the previous paper (**4**):

$$(1) \qquad K = 4 \cdot m \cdot \frac{y'_o}{t_o^2}$$

where K = side force (in dynes) acting at a constant rate in the Y-direction during the travel of the cube aggregate on the horizontal plane;

m = mass (in grams) of the cube aggregate for one side (A or B) as target;

y'_o = half difference (in centimeters) between the centers of gravity of the two aggregates;

t_o = travel time (in seconds) on the horizontal plane.

It is convenient to state the value of K for a single cube. In this case m in the formula is the mass of *one* cube.

The travel time t_o has to be measured. It is different for different cube types, and it varies—for instance with the size, weight, and surface conditions. It is also dependent on the number of cubes released simultaneously; and if the aggregate is made up of cubes of different kinds, then the travel time for a certain kind may differ from the value found for an aggregate which comprises only this kind. The measurement of t_o cannot be made accurately without complicated and expensive instruments; but if a value of t_o has been established for certain conditions, then it is possible to calculate t_o for other conditions on the basis of a measurement of the travel dis-

tance x_o in the running direction. This involves an assumption, however, a very plausible one, namely, that the velocity u_o with which the cubes enter the horizontal plane is the same in all cases. This velocity is primarily determined by the acceleration due to gravity, because the cubes fall a distance in the air before hitting the incline; and as the velocity does not exceed two meters per second, the speed differences which may be caused by differences in the air resistance are negligible. A series of measurements for wood cubes gave a mean t_o value which is very near .5 seconds with a standard deviation approximately .014. The corresponding value of x_o was measured at 46.5 cm. The velocity u_o when the cubes enter the horizontal plane then with be

$$(2) \qquad u_o = \frac{2x_o}{t_o} = \frac{2 \cdot 46.5}{.5} = 186 \text{ cm./sec.}$$

We then may calculate t_o for other conditions, after having established a value for x_o, on the basis of the formula

$$(3) \qquad t_o = \frac{2x_o}{u_o} = \frac{2x_o}{186} = \frac{x_o}{93} \text{ sec.}$$

Results

Section I (Series S26, S27)

The new research was started with an investigation not directly connected with the dynamical one, although it may be of some general interest. It was questioned whether aberrations of the same high magnitude as in Throw 1 in standard series with sets of five releases for each side as target would occur also in series with sets comprising only one release for each side. Series S26, with six wood cubes per release, was carried out with the object of investigating this point. It comprises ten sets, which in this case comprise only one throw of six dice for each side. These were carried out at the rate of one such set a day. All sets except one gave a positive difference for $2y'_o$. The mean *B-A* difference in the 120 trials is 10.31 cm. in the expected direction, with $t = 2.97$, and $P = .003$.

The difference of 10.31 cm. is somewhat smaller than the 12.56 cm. obtained in Throw 1 in the previous series (S1) with wooden cubes. Perhaps this result may have been influenced by the fact that in S26 the double throw in the set was started with the *B*-side as

target, whereas the general rule in these experiments is to start with the *A*-side as target.

The object in the next series, S27, was the comparison of the results with rough bakelite cubes and aluminum cubes, three of each kind being released simultaneously. In this series, ten two-throw sets were again carried out, but the set was started with the *A*-side as target and no ten-throw sets were done for comparison. Readings were taken in both *X*- and *Y*- directions, thus permitting the calculation of the side force *K*. This is the first series in which the dynamical behavior of aluminum cubes was investigated, and it gave some surprising results.

In Table 1, which contains the statistical evaluation of all series in this new research, it is seen that *both* materials in S27 gave a positive *B-A* difference, although the series represented a dual-aspect task. This is in contradiction to the results obtained in the bakelite series S19 + S22 + S24 + S25 where, in Throw 1, the rough cubes went to the target side, but the smooth ones, which were released together with the rough cubes, went to the nontarget side. We may be confronted here with psychological differences in the two series, but we cannot exclude a priori that the physical differences of smooth bakelite cubes and aluminum cubes may be a point to consider in this connection.

The basic data used for the calculation of the side force *K* for all the series are given in Table 2, and the side force for the different cube materials is given in Tables 3 and 4. It is seen in Table 3 that the mean side force *K* for the rough bakelite cubes in Series S27 is near the same as that previously found for this kind of cube in S19 + S22 + S24 + S25, namely, 237 dynes, as compared with the previous value of 241 dynes. The difference is 1.5 percent. The corresponding value for the aluminum cubes in S27 is 692 dynes. Thus the mean side force for the bakelite cubes and the aluminum cubes in combination is 465 dynes.

This high *K*-value does not fit well with the theory advanced in the previous paper (6), that the subject is responsible for a certain, and rather constant, amount of force, with the effect that the aberration of the cubes is inversely proportional to their mass. According to the results in the new series it would appear that the *cube material*

Table 1

RESULTS OF SERIES S26-S41 IN TERMS OF B-A DIFFERENCES

Section	Series	Type of Cubes	A-Target		B-Target		B-A Diff.		
			No. of Trials	Average Position (in cm.)	No. of Trials	Average Position (in cm.)	$2y'_0$ (in cm.)	t_d	P
I	S26	wood cubes (pine)	60	33.37	60	43.68	+10.31	2.97	.003
	S27	rough-2 bakelite cubes	30	35.37	30	41.73	+ 6.36	1.68	.1
		aluminum cubes	30	37.90	30	42.50	+ 4.60	1.92	.06
		both kinds, average	60	36.64	60	42.12	+ 5.48	2.46	.014
II	S28 +S29 +S30 +S32	aluminum cubes total series	600	38.49	600	40.43	+ 1.94	2.72	.006
		Throw 1 in unit	120	36.48	120	42.26	+ 5.78	3.67	.00025
	S31	alum. cubes ("control") total series	150	41.01	150	40.29	− .72	....	.....
		Throw 1 in unit	30	39.97	30	37.90	− 2.07	....	.....
	S28 +S29	aluminum cubes total series	300	37.79	300	40.04	+ 2.25	2.12	.033
		Throw 1 in unit	60	36.48	60	43.75	+ 7.27	3.21	.0014
III	S33 +S34	wood cubes (beech) total series	300	39.25	300	40.03	+ .78	....	.....
		Throw 1 in unit	60	35.15	60	41.72	+ 6.57	3.07	.002
	S35 +S36	wood cubes (beech), alum. coating, 8μ total series	300	38.08	300	40.42	+ 2.34	not calculated	
		Throw 1 in unit	60	33.12	60	42.42	+ 9.30	3.70	.00022
	S37 +S38	wood cubes (beech), alum. coating, 150μ total series	300	36.66	300	39.46	+ 2.80	not calculated	
		Throw 1 in unit	60	31.53	60	42.73	+11.20	5.28	abt. 10^{-7}
IV	S39 +S40	aluminum cubes, paper coating, 190μ total series	300	37.35	300	39.91	+ 2.56	not calculated	
		Throw 1 in unit	60	32.45	60	41.93	+ 9.48	3.71	.00022
	S41	alum. cubes ("control") paper coating, 190μ total series	150	40.47	150	39.55	− .92	....	.....
		Throw 1 in unit	30	43.73	30	36.30	− 7.43	2.64	.008

likewise is a factor in these experiments. It is interesting in this connection that the rough bakelite cubes in S27 have given practically the same *K*-value as in the previous series. The high *K*-value of 692 dynes with aluminum cubes cannot be attributed to a high degree of roughness of the surface, because the friction measure-

Table 2

BASIC DATA FOR THE CALCULATION OF THE SIDE FORCE K IN SERIES S27 TO S40 WITH 16 MM. CUBES

Series	S27		S31*	S33+S34	S35+S36	S37+S38	S39+S40
Material	Rough-2 Bake-lite	Alum-inum	Alum-inum	Wood (beech)	Wood (beech), 8μ alum. coating	Wood (beech), 150μ alum. coating	Alum-inum, 190μ paper coating
No. of readings	60	60	300	600	600	600	600
Weight (grams per cube)	5.6	10.3	10.3	2.6	2.64	3.23	10.7
Friction coeff.	.44	.38	.38	.51	.49	.41	.53
Mean travel distance x_o (cm.)	50.9	34.4	38.7	37.8	40.3	38.0	41.3
Mean travel time t_o (sec.)	.548	.370	.416	.406	.434	.409	.444
K (dynes per cube)	$74.5y'_o$	$301y'_o$	$238y'_o$	$63.2y'_o$	$56y'_o$	$77y'_o$	$218y'_o$

*These data are used for the calculation of the side force K in aluminum series S28, S29, S30, and S32.

ments have given rather low values for the friction coefficient with these cubes.

Section II (Series S28-S32)

Series S27 being rather short, there was a need for carrying out further series with aluminum cubes in order to investigate whether the high K-value would be confirmed. Four standard series were conducted with six aluminum cubes per release, S28, S29, S30, and S32. Further, a "control" series, S31, was carried out in which the subject made no conscious attempt to influence the dice. In the four placement series, readings were taken only in the Y-direction, but in the "control" series, in the X-direction as well. The mean traveling distance x_o in the "control" series is used for the calculation of the side forces in the placement series.

It is seen in Table 3 that the mean side force in the four aluminum series is 688 dynes, which is of the same order of magnitude as the value found in S27 for the aluminum cubes (692 dynes).

When separately evaluated, the four aluminum series give the K-values 881, 849, 540, and 485 dynes respectively, suggesting a chronological decline within the experiment. *All* the four B-A dif-

Table 3

SIDE FORCE *K* AS CALCULATED ON THE BASIS OF THE OBSERVED ABERRATION IN THROW 1 IN THE FIVE-THROW UNIT

Series	Cube material	ABERRATION (CM.)			SIDE FORCE K (DYNES PER CUBE)		
		Target		Average	Target		
		A	B	(y'_0)	A	B	Average
S1	wood (pine)	+8.42	+4.14	+6.28	270	132	201
S19+S22	rough-2 bakelite	+4.57	+2.42	+3.50	315	167	241
+S24+S25	smooth bakelite	−2.50	−1.18	−1.84	225	105	165
S27*	rough-2 bakelite	.....	.....	+3.18	...	...	237
	aluminum	.....	.....	+2.30	...	...	692
S28+S29	aluminum						
+S30+S32		+2.98	+2.80	+2.89	709	667	688
S28+S29	aluminum	+2.42	+4.84	+3.63	577	1153	865
S33+S34	wood (beech)	+4.49	+2.08	+3.29	283	131	207
S35+S36	wood (beech), 8μ alum. coating	+6.13	+3.17	+4.65	343	177	260
S37+S38	wood (beech), 150μ alum. coating	+6.53	+4.67	+5.60	502	358	430
S39+S40	aluminum, 190μ paper coating	+6.18	+3.30	+4.74	1350	720	1035

*In S27 the set contains only *one* throw for *A*, and *one* for *B* as target.

ferences are in the expected direction. The results in the five-throw unit for the four series are shown graphically in Figure 1. The corresponding values in the "control" series S31 are included in the figure. They show trends in relation to the placement results similar to those observed in earlier "control" series, as for instance a reversal of the effect in Throw 1 in the five-throw unit.

Section III (Series S33-S38)

After having obtained confirmation of the high *K*-value with aluminum cubes the problem was reconsidered in order to establish a line for the continued research.

From the results in Series S19 + S22 + S24 + S25 with bakelite cubes, there were indications that the cube surface has an influence on the results, because the rough cubes show about 50 percent higher

Table 4

SIDE FORCE *K* (AVERAGE FOR BOTH TARGETS A AND B IN THROW 1) AS RELATED TO THE PHYSICAL PROPERTIES OF THE CUBES

Cube material	Wood				Bakelite		Aluminum	
	Pine	Beech						
Cube surface	wood	wood	alum. foil 8μ	alum. foil 150μ	glue and sand	bakelite varnish	alum.	paper 190μ
Friction coeff.	.47	.51	.49	.41	.44*	.38	.38	.53
Cube weight (grams)	2.0	2.6	2.64	3.23	5.6	5.6	10.3	10.7
Side force K (dynes per cube) in Series:								
S 1	201							
S19+S22+S24+S25					241	165		
S27					237		692	
S28+S29+S30+S32							688	
S28+S29							865	
S33+S34		207						
S35+S36			260					
S37+S38				430				
S39+S40								1035

*This figure was found by measurements carried out after Series 527. The previous figure .49, given in the paper (6), refers to measurements carried out after Series 519. The cube surface apparently has been subject to some wear during the experiments.

side force than the smooth ones. This circumstance was previously considered to be due to the higher friction with the rough cubes. But the results in the series with aluminum cubes, which, as measurements have shown, have the same friction coefficient as the smooth bakelite cubes, suggested that we cannot be sure that the mechanical friction of the surface is in reality a factor. The difference in the *K*-values with the two kinds of bakelite cubes therefore can alternatively be attributed to the *differences in surface material.* The rough cubes have a coating of glue and sand, whereas the surface of the smooth cubes consists of bakelite varnish. These considerations made it desirable to pay special attention to the *surface material* when continuing the experiments.

A promising way seemed to be to provide wood cubes with a coating of aluminum. With such cubes, higher aberrations than with pure wood cubes could be expected if the postulate concerning the influence of the surface material be right, because the aluminum cubes had given outstanding results.

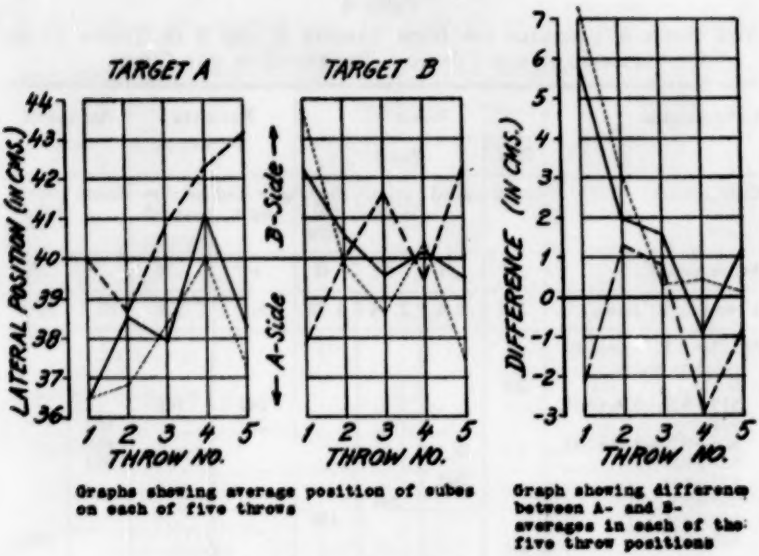

FIG. 1. Results in series with aluminum cubes.
Solid line: Placement Series S28 + S29 + S30 + S32
Broken line: "Control" Series S31
Dotted line: Placement Series S28 + S29

In continuation, series were conducted with new wood cubes of beech (the previous wood cubes were of pine). In Series S33 + S34 they were uncoated, and they gave in Throw 1 in these series practically the same *K*-value as the previous pine cubes, 207 dynes with beech as compared with 201 dynes with pine.

Then the beech cubes were provided with an aluminum cover with a thickness of 8 μ, and Series S35 + S36 were carried out with these cubes. The *K*-value was then found to be 260 dynes. It is higher than for beech cubes, but far below the value for pure aluminum cubes. It therefore was suspected that not the surface alone is responsible for the effect, but that the material in the cube interior may be of influence to some depth.

The aluminum coating therefore was increased to 150 μ in thickness, and two more series, S37 + S38, were carried out. A substantial increase in the side force *K* was then obtained. It reached a value of 430 dynes, which is about 50 percent of that for pure

aluminum cubes, whereas the value for pure beech cubes is 24 percent of the value for aluminum cubes. With aluminum cubes the mean value of 865 dynes in the first two series, S28 + S29, then has been taken as a basis because it is safer to compare values gained in series with the same number of trials, in order to minimize the bias due to chronological decline.

The results in Series S33 to S38 with uncoated and aluminum coated beech cubes are shown graphically in Figure 2.

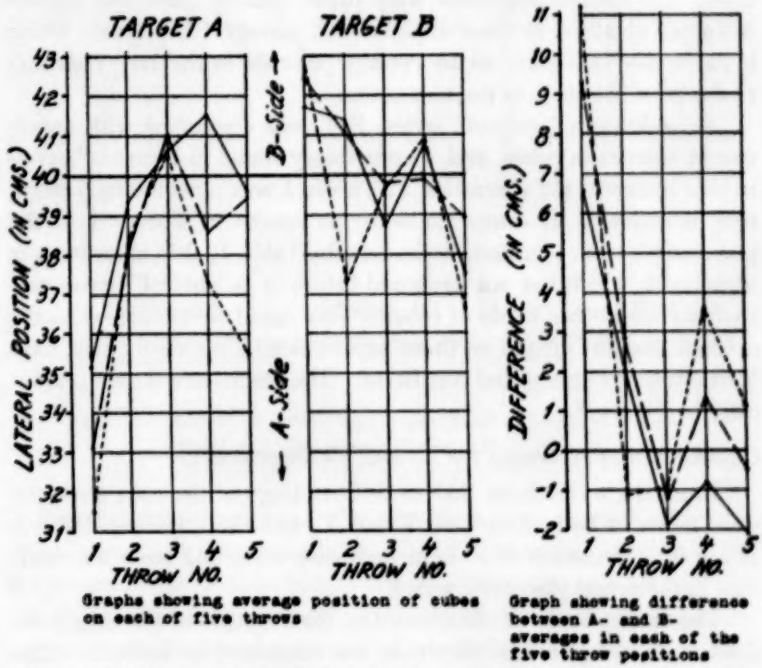

FIG. 2. Results in placement series with uncoated and coated beech cubes.
Solid line: uncoated, Series S33 + S34
Broken line: 8μ aluminum coating, Series S35 + S36
Dotted line: 150μ aluminum coating, Series S37 + S38

Section IV (Series S39-S41)

At this point of the investigation it seemed rather clear that the effect on the cubes got stronger the deeper the aluminum layer reached beneath the cube surface. This circumstance brought up the

question whether the effect on aluminum cubes would be reduced if the cubes were provided with a nonmetallic coating, such as paper. In order to investigate this point two more series, S39 + S40, were carried out, using aluminum cubes with a paper coating of 190 μ thickness. The expectation was that this coating would reduce the effect so that considerably smaller K-values would be obtained than with uncoated aluminum cubes.

The experimental results were in contradiction to this expectation. The aluminum cubes with paper coating gave the highest K-values obtained in these experiments, namely 1035 dynes, which is more than one gram on an average per cube in the 120 trials carried out in Throw 1 in the two series.

In addition a "control" series, S41, was conducted with paper-coated aluminum cubes, and, as previously found in "control" series in this research, the aberration in Throw 1 was outstandingly negative in direction as compared with the results in Throw 1 in the placement series. In fact, as is seen in Table 1, this aberration is significant, which has not happened before in a "control" series carried out with other kinds of cubes. This result lends support to the opinion that the effects on these paper-coated aluminum cubes have been unusually strong and consistent. The results are shown graphically in Figure 3.

Comparison of Averages for X- *and* Y- *Directions*

In all the series from S33 to S40 readings of the cube positions were taken in both directions X and Y, and the traveling distance x_0 for the calculation of K is in each case computed from the readings in the actual placement series.

The results in the X-direction, i.e., the averages of the cube positions in the longitudinal direction, are contained in Table 5. The subject made no conscious attempts to influence the cubes in this direction, so the target designations for the X-figures are put in quotation marks. It is interesting to compare the differences between the averages, $2x'_0$ with those obtained in the Y-direction, $2y'_0$ (also included in Table 5), in which direction the "influence" was aimed. In the Y-direction the difference is positive throughout, and the aggregate value, +8.41 cm. in 600 trials, is highly significant. In the X-direction the difference is positive in five series and negative

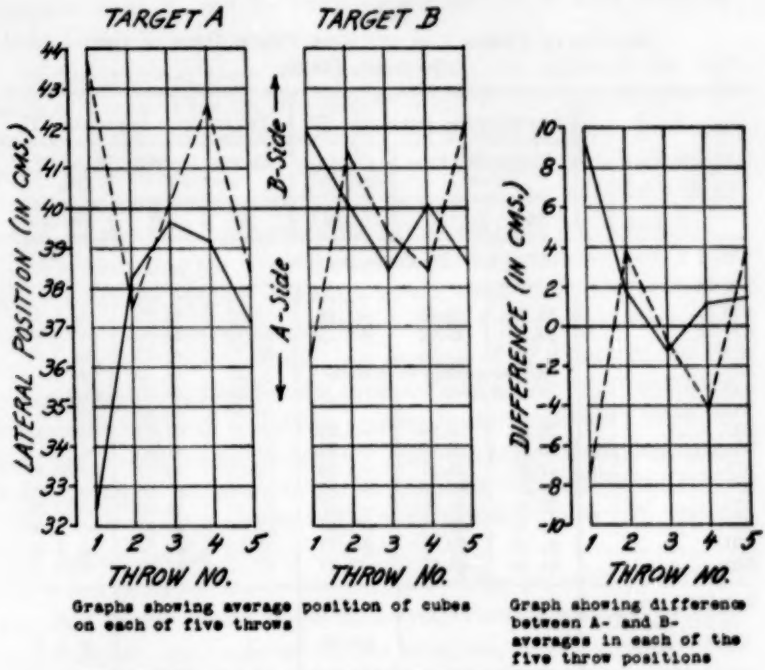
TARGET A
TARGET B
LATERAL POSITION (IN CMS.)
B-Side
A-Side
THROW NO.
THROW NO.
Graphs showing average position of cubes on each of five throws
DIFFERENCE (IN CMS.)
THROW NO.
Graph showing difference between A- and B-averages in each of the five throw positions

FIG. 3. Results in series with paper coated aluminum cubes.
Solid line: Placement Series S39 + S40
Broken line: "Control" Series S41

in the remaining five series. The aggregate value in the *X*-direction, +1.13 cm., is at the chance level. It is less than one seventh of the difference in the *Y*-direction. The X-differences in S38 and S39 are significant at the P-level of about one percent, and thus they suggest an unconscious influence of some kind. But the interesting point in the comparison is that no order can be traced in the effects as concerns the *X*-direction, whereas there is a very pronounced order in the *Y*-direction, in which there were strong conscious attempts to obtain positive differences (the results being positive in all series). This would seem to strengthen the opinion gained from earlier observation (for instance the reversal of the effects in "control" series as compared with those in placement series) that the *conscious* efforts exerted by the subject play a dominant role in

Table 5

RESULTS IN THROW 1 IN THE FIVE-THROW UNIT IN THE INDIVIDUAL SERIES

Series	LONGITUDINAL DIRECTION (X)			TRANSVERSAL DIRECTION (Y)		
	Average Position (in cm.)		"B-A"-Diff. $2x'_o$	Average Position (in cm.)		B-A-Diff. $2y'_o$
	"A"	"B"	(in cm.)	A	B	(in cm.)
Placement Series						
S27 I	51.83	50.03	−1.80	35.37	41.73	+ 6.36
S27 II	33.83	35.00	+1.17	37.90	42.50	+ 4.60
S28				35.77	43.17	+ 7.40
S29	no X-recordings were made			37.20	44.33	+ 7.13
S30	in these series			36.20	40.73	+ 4.53
S32				36.73	40.80	+ 4.07
S33	36.83	34.90	−1.93	35.67	42.03	+ 6.36
S34	36.57	39.10	+2.53	34.63	41.40	+ 6.77
S35	42.47	38.67	−3.80	33.00	43.17	+10.17
S36	39.93	40.90	+ .97	33.23	41.67	+ 8.44
S37	36.97	34.30	−2.67	31.40	44.60	+13.20
S38	38.33	47.70	+9.37	31.67	40.87	+ 9.20
S39	41.00	50.77	+9.77	31.73	40.57	+ 8.84
S40	46.30	43.97	−2.33	33.17	43.30	+10.13
B-A-Diff., average in S27, and S33 to S40			+1.13			+ 8.41
"Control" Series						
S31	40.20	42.30	+2.10	39.97	37.90	− 2.07
S41	40.60	39.53	−1.07	43.73	36.30	− 7.43
B-A-Diff., average			+ .54			− 4.75

obtaining positive results. The conclusion indicated is that the efforts are effective only in the intended direction.

This question was previously raised by Dr. Carroll B. Nash, but it could at that time be answered only on the basis of limited material (7). Since then several more series have been conducted; and, as shown above, the further results seem to confirm the previous finding that the distance the die rolls in the longitudinal direction is not affected by the distance it is displaced in the lateral direction. This, according to Dr. Nash, would represent a proof of the hypothesis that PK impels the die the distance it is laterally displaced.

Comparison of A- *and* B- *Targets with Respect to Side Force in Throw One*

In the previous paper (**6**) a comparison was made of the side forces in Throw 1 for the two targets *A* and *B*, based on the aberrations from the total series average. As is seen in Table 7 of that paper, the side force *K* for the wood and bakelite cubes with *A* as target was consistently about twice as large as the force with *B* as target, or about 66.7 percent of the aggregate force for *A* and *B*.

When starting the new experiments with aluminum cubes, I was interested to see whether this effect would repeat itself. After having terminated the first aluminum series, S28, I was surprised to find that the effect was reversed. An average of 67.0 percent for the *A*-target had been found in the previous series with wood and bakelite, but in S28 with aluminum practically the same value, 67.4 percent, was found for the *B*-target. This was rather confusing, and I was anxious to see what the result would be in the following series, S29. This series gave almost the same distribution as S28, namely 65.8 percent for the *B*-target.

When studying the problem I recalled the experiences with the so-called "order-effect" in the earlier experiments with the two-area arrangement. By "order-effect" we have designated the much higher deviation arrived at in sets started with the *A*-side as target than in those started with the *B*-side as target. This effect was subsequently found in extensive experimental material, but it disappeared when watched for in the continued experiments. However, after a time, when my interest in the effect had ceased, it appeared again in further series. Concerning this effect the reader is referred to the original papers (**2, 3**).

As the *A*/*B*-distribution of the aberration in the actual series is a kind of *secondary* effect obviously similar to the "order-effect," and as I have found that such secondary effects seem to be considerably more likely to be affected by observation than the *primary* effect which is consciously aimed at in the experiment, I decided not to pay attention to the *A*/*B*-distribution in the further series; i.e., in S30 and the following. A continued investigation of the distributions thus was first undertaken only after Series S41 was finished. How this procedure worked out is seen from the data in Figure 4 and

Table 6. These findings have to be interpreted in relation to the general physical balance of the apparatus as regards the favoring of the two sides, and the results for throws 2-5 shown in Figures 1-3 give a reasonable assurance that there was no marked favoring of one side. Also, the stronger favoring of the *B*-side for Throw 1 in S28, S29, and the "control," S31, suggests the psychological interpretation.

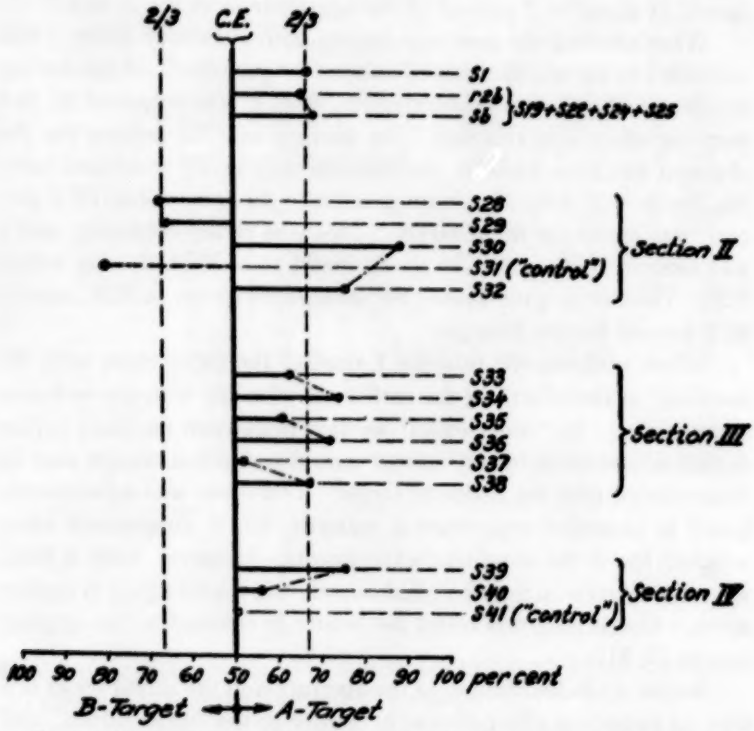

FIG. 4. Percentage on target of total side force measured in the first throw of the five-throw unit.

In the first series in which attention to the distribution was no longer given, S30, the *A*-target is again favored, the percentage being as high as 88.5. In the following series, S31, which is a "control" series, there is a shift in favor of the *B*-side, with 80.2 percent of the total aberration in Throw 1 for this side. But then, in the next

Table 6

PERCENTAGE ON TARGET OF TOTAL SIDE FORCES MEASURED IN THE FIRST THROW OF THE FIVE-THROW UNIT

Series	Cube Material	TARGET	
		A	B
S 1	wood (pine)	67.2	32.8
S19+S22+S24+S25	bakelite	66.8	33.2
	Average, S1, and S19 etc.	67.0	33.0
S28+S29	aluminum	33.4	66.6
S30+S32	aluminum	82.0	18.0
S33+S34	wood (beech)	68.4	31.6
S35+S36	" with 8μ alum. coating	66.6	33.4
S37+S38	" " 150μ " "	59.7	40.3
S39+S40	aluminum with 190μ paper coating	65.8	34.2
	Average, S30 to S40	68.0	32.0

In S28 and S29 the force distribution on *A* and *B* was kept under scrutiny. In the further series no attention was given to the distribution during the experiments. For this reason the values in series S28+S29 were not considered when computing the average.

placement series, S32, we have again the larger aberration for the *A*-target, namely 75.5 percent. The swinging-over of the effect to the *B*-side in the intermediate "control" series, S31, is in accordance with the usual observations (reversal of the effects) in "control" series as compared with placement series. The average in the two placement series S30 and S32 is 82.0 percent in favor of the *A*-target.

In *each* of the eight following series, S33 to S40, there is a preponderance on the *A*-side in the distribution of the side force. In Table 6 the averages for each two series are given. When forming the average for the ten series S30 to S40 (excluding the "control" series S31) a value of 68.0 percent is found for the distribution in favor of the *A*-side as target. This is practically the same average as previously found for the wood cubes (Series 1) and bakelite cubes (Series S19 + S22 + S24 + S25) namely 67.0 percent, in favor of the *A*-side. Thus in two extensive experiments, when this special distribution effect was not watched for, we have arrived at the same average distribution of the side force.

Reviewing the situation we can say that the considered distribu-

tion effect was very consistent as long as it occurred without conscious observation in the experiments with wood and bakelite cubes. But when making it an object of observation in S28 and S29, it was reversed in a striking way. When the conscious observation was dispensed with in the following series the old 2/3-distribution in favor of the *A*-side was reëstablished, giving the impression that this distribution is in some way the natural one in the present experiments. A reason for this might perhaps be seen in the circumstance that my location relative to the apparatus is always on the *A*-side.

The experiments made here concerning the distribution effect may have a bearing on the question of the repeatability of PK experiments or of psi experiments in general. They show that the factor of conscious observation is one which must be duly considered—at least when secondary, unintended effects in the results are concerned.

General Summary and Discussion of Results

A *t* test has been made of the combined results in Throw 1 of the "scaled" placement experiments dealt with so far in these reports. In the placement series S1, S3, S19, S22, S24-S30, and S32-S40, the difference between the averages for *B*- and *A*-targets amounts to +7.61 cm. The total number of trials in Throw 1 is 1452. The difference has a *t* of 10.45, corresponding to a P of less than 10^{-20}.

A further *t* test was carried out on the combined results in Throw 1 of the "control" series S2, S31, and S41. The average of the difference between *B*- and *A*- "targets" is −4.79 cm., which has a *t* of 2.71, and a P of .0065 in the 252 trials involved.

It appears from these results that there must be a substantial difference in the two situations: (a) when conscious attempts are made to influence the dice; and (b) when the subject is mentally neutral.

The fact that the result in the "control" series is significant in itself gives weight to the opinion that there is a back-effect on the subject from the placement series, but it is difficult to distinguish whether this effect is directly due to his repeated *mental efforts*—as suggested in a previous report (**5**)—or to his *knowledge* about the positive results in the preceding placement series.

It has been pointed out by W. Pauli (**8**, p. 166) that a back-effect of the obtained knowledge on the observer is a situation which falls

outside the scope of natural science. However, the repeated occurrence of the reversal of effects in "control" series as compared with placement series apparently shows that this statement does not apply to *all* types of experiments.

It is useful to present the following summary of the material as concerns the obtained side force *K:*

a. The series with *wood* cubes show no substantial mutual differences. With *beech* cubes in S33 + S34 almost the same *K*-values were found as with *pine* cubes in S1, thus:

		A-Target	*B*-Target	Average
S1	pine	270 dynes	132 dynes	201 dynes
S33 + S34	beech	283 dynes	131 dynes	207 dynes

b. In the new series, S27, the *rough bakelite* cubes have given practically the same *K*-value as obtained with this kind of cube in Series S19 + S22 + S24 + S25; i.e., 237 dynes, as compared with the previous value of 241 dynes.

c. The unexpected high *K*-value of 692 dynes with *aluminum* cubes in S27 was confirmed in Series S28 + S29 + S30 + S32, which gave an average of 688 dynes. If we consider only the first two series, S28 + S29, the side force amounts to 865 dynes.

d. A gradual increase in the side force *K* was found when the beech cubes were provided with *coatings of aluminum*. And the values tend to a maximum with pure aluminum cubes, as is seen in the following table.

S33 + S34 pure beech cubes	K = 207 dynes
S35 + S36 beech cubes with 8 μ aluminum coating	K = 260 dynes
S37 + S38 beech cubes with 150 μ aluminum coating	K = 430 dynes
S28 + S29 pure aluminum cubes	K = 865 dynes

e. The side force in Series S39 + S40 with *aluminum* cubes provided with *paper coating* was 1035 dynes, which exceeds the value found for uncoated aluminum cubes.

One point has to be mentioned here concerning the calculation of *K*. This calculation is as a rule based on the running time t_o computed from the mean distance of movement x_o for the *entire* series, comprising all throws 1 to 5, whereas the calculated *K*-values refer only to Throw 1. Apparently it is more accurate to base the calculation of *K* in Throw 1 on x_o for Throw 1, because this x_o may show variation relative to the total average. The reason why this has not

yet been done is that regular recordings of both x and y in the placement series were not started until Series S33. In order to get a comparison of the K-values on a *uniform basis*, it was therefore necessary to base the calculation in Series S33 and the following also on the total average of x_o.

Table 7

SIDE FORCE K IN THROW 1 (AVERAGE FOR A- AND B-TARGETS) AS CALCULATED ON THE BASIS OF THE TRAVELING DISTANCE x_o IN THROW 1 IN THE ACTUAL SERIES

Series	x_o (cm.) Average in Throw 1	t_o (sec.)	y'_o (cm.)	K (dynes)	K (dynes), when calculated on the basis of x_o for the *entire* series
S33+S34.....	36.85	.396	+3.29	218	207
S35+S36.....	40.49	.436	+4.65	259	260
S37+S38.....	39.33	.423	+5.60	404	430
S39+S40.....	45.51	.489	+4.74	847	1035
S41.....	40.07	.431	−3.72	858	809

We have given in Table 7, for Series S33 to S41, the side force K in Throw 1, based on x_o for Throw 1, and it is seen that there are certain differences from the previous results, although not so great that they change the mutual order of magnitude of K in the different experiments. Most pronounced is the difference in S39 + S40 with paper-coated aluminum cubes, where the K-value is reduced to practically the same value as that found in S28 + S29 with pure aluminum cubes. However, these values are not directly comparable, because in S28 + S29 the calculation is based on x_o for the entire "control" series S31.

In Table 7 the K-value is also given for the "control" series S41, and an interesting point is that with the more accurate method of calculation the side force is practically the same in the "control" series as in the preceding placement series, S39 + S40, with the same paper-coated aluminum cubes, although the "force" naturally has acted in the opposite direction in the "control" series.

We have here to deal also with the opinion held by some investigators that the differences in K observed with different types of cubes may not be attributed to the *physical* variations, but may be due to

psychological differences—conscious and unconscious—in the various experiments.

The question is a delicate one in view of the circumstance that we have no way to specify quantitatively the strength of the mental efforts exerted by the subject. However, it is not sure that this is a necessary condition for dealing with the question whether the psychological or the physical conditions are responsible for the differences. When following the positivistic method of investigation it would seem to be most convenient not to consider the hypothesis concerning the differences in the psychological conditions since it is one of speculative character and difficult to verify, but to search first for lawfulness in the results without considering any special hypothesis. There are, however, in the experimental material obtained so far, strong indications that we are confronted with rather constant conditions as concerns the subject's mental efforts, at least as far as Throw 1 in the five-throw unit is concerned. We have dealt extensively with this question in the previous report (**6**, p. 150), mainly on the basis of the parabolic distribution of hits in the five-throw unit in earlier series. According to the results in the present report, however, it seems that we will have to make an adjustment in the working hypothesis presented in that paper, which hypothesis assumes the occurrence of PK forces of the same magnitude with cubes of different kinds. It looks as if it is no longer correct to talk about *PK forces* in this connection, but rather of ***mental efforts***. The new experimental material suggests that the PK forces are not dependent on the subject's efforts alone, but also on the cube material. This would mean that the energy released in the experimental situation is a function both of the subject's mental efforts and of properties connected with the cube material.

A working hypothesis along these lines would seem to be a promising one to follow in the experiments to come. Accordingly it seems useful in the first place to assume the efforts in Throw 1 as being constant from series to series, because there is justification for this assumption in the experimental results.

The material so far presented in these reports might still be too limited to provide a sufficient basis for more extensive studies concerning lawfulness in the relationship between the forces and the

physical conditions. But the results have given some interesting cues for further research. It seems obvious that more attention must be given to the cube material and its various properties.

Before terminating this report, I should like to give some data from a further series, S42, carried out by a German physicist, Heinz Voigt of Frankfort, during a visit to Sweden last year.

H. V. acted as the subject and handled the whole experiment himself in my presence. I assisted him only in checking the readings, and in making the records. The beech cubes with 150 μ aluminum coating were used. The series comprised ten standard sets, carried out in five sessions. The *B-A* difference for the whole series, +.53 cm., is at the chance level. Throw 1 shows the greatest difference of the five throws in the unit, +4.38 cm., but it is not significant. However, when the five sets which were carried out as the first ones in the session are considered separately the picture looks different. In Throw 1 in these sets the *B-A* difference is +8.37 cm. A *t* test of this difference for the 60 trials involved gives $t = 2.95$, with $P = .0032$.

Readings were taken in the *X*-direction as well, giving an average for the entire five sets of $x_o = 38.87$ cm., and for Throw 1 alone an average of $x_o = 39.35$ cm. The corresponding side force in Throw 1 is K = 309 and 302 dynes, respectively. The total *B-A* difference of the five first sets is +1.70 cm., which is at the chance level.

The distribution of the *B-A* difference in Throw 1 on the two target sides, based on the total y_o average in the five first sets, shows a favoring of the *A*-side to the degree of 64.7 percent, a result which is in accordance with my own. H. V., like me, had his place on the *A*-side of the apparatus during the experiments.

The side force *K*, when based on the actual x_o in Throw 1 (302 dynes), is 75 percent of the value (404 dynes) I obtained with these same cubes in experiments comprising only one set per session. However, it is necessary to take into account the fact that the psychological conditions were not the same in the two cases. It is therefore not advisable to compare the *K*-value of S42 in the further investigations into the dynamics of the PK placement situation.

The results in Series S42 were reported here mainly in order to

show that the same trends in the results have been found in this experiment with another person as the subject, as in those in which I worked alone as my own subject.

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TIME ALIVE: J. W. DUNNE AND J. B. PRIESTLEY

By Grover Smith, Jr.

[The imaginative construction of a plot by the novelist or playwright may well involve some of the same rational operations which the exploratory scientist exercises in his search for hypotheses or possible answers to the question he is seeking to answer. Ideally the artist may well be expected to be able to explore more widely, to transcend his barriers more freely, and to perceive the pattern of things more insightfully than the more systematic and cautiously moving scientist. It is likely that the writer, especially, in personifying his ideas as characters and thereby gaining the advantage of more concrete operations and more realistic interactions, may be able to follow up the possibilities of those ideas more successfully than the more abstract-minded research worker himself.

For some decades now the question has been under scientific consideration as to whether the human mind can disengage itself from what is called the present and invade the hypothetical realm that we loosely refer to as the future. Since the very terms relating to time are themselves somewhat vague and the problem of precognition is complex enough to allow for a range of hypotheses, the dramatist has found this unreal territory of the future appropriate ground on which to play with the interesting possibilities that are conceivable. Students of parapsychology who are interested in precognition may find stimulation and interest in Dr. Grover Smith's article "Time Alive: J. W. Dunne and J. B. Priestley" published in the *South Atlantic Quarterly* of April, 1957, and accordingly it is reprinted here with permission of the author and the publishers, Duke University Press.—Ed.]

MUCH has been written in the twentieth century on the subject of time. In creative literature, ideas with a dubious philosophical pedigree, such as those of Bergson on *durée*, sometimes achieve artistic respectability, as his have done in the work of Proust. This paper, concerned not with the enthusiastic Bergson, who, by the way, disbelieved in time, but with J. W. Dunne, who believed in it with extraordinary faith, proposes to trace the influence of his peculiar theory on J. B. Priestley, a playwright and novelist of a minor stature. The time-writings of Priestley are the most accessible

and tractable imaginative material exhibiting Dunne's philosophy. Dunne was scarcely a great philosopher; but Priestley illustrates the force of Dunne's speculations, and better than any of his contemporaries, including makers of science-fiction, helps to clarify much that is bewildering in the present-day literature of time.

J. W. Dunne may be likened to the Eleatic philosopher Zeno, as Bergson has been likened to Zeno's predecessor Heraclitus. Heraclitus, the philosopher of flux, once remarked that one can never step twice into the same river or be twice the same person attempting to do so. Heraclitus found the world in a state of perpetual change. Zeno, on the other hand, echoing his master Parmenides, taught that change—in fact elemental motion of any sort—was impossible. To Zeno, what people saw as movement was actually a succession of unmoved states. His celebrated paradox of the flying arrow which does not fly, because it is in reality a sequence of motionless arrows tip to tip, has fortified in the minds of many the idea that time might be a dimension. Dunne, for his own dimensional view of time, did not have to go to Zeno. He pointed out in 1927 that forty years previously time had been treated as a fourth dimension by C. H. Hinton. The phrase "fourth dimension" had, meanwhile, acquired some popular repute through being applied in a similar way by the Relativitists. In Dunne's writings (*An Experiment with Time,* 1927; *The Serial Universe,* 1934; *The New Immortality,* 1938; *Nothing Dies,* 1940) the fourth dimension, instead of being the only simultaneous tract of time, is but the first of an infinite series, each of which may be said to *time* the next preceding. For this serial conception (rather than for his having designed and flown the first British military airplane) Dunne is now remembered.

One might begin an exposition of Dunne's views at various points. Zeno's arrow suggests one analogy. Let us think of the arrow as lying stationary on the ground. Its duration in a fourth dimension of real time may be represented in a diagram precisely as Zeno represented its apparent movement through space. That is, it may be pictured as a line of arrows, each one being seen at a different moment. All look like the same arrow, though they occupy separate moments of observation, just as in a moving picture a

succession of still photographs of a man gives the illusion that he endures through the time it takes to watch them. But what imparts to these arrows the appearance of movement? If the fourth-dimensional duration, like the photographs on the film-strip, is really static, can time—that is, the fourth dimension itself—be considered to time itself? Dunne's answer is No. It is necessary, he says, to regard the fourth dimension as timed by a fifth dimension perpendicular to it. As the three dimensions of space move, or seem to move, through time, so time moves, or seems to move through a secondary time, and this through a tertiary, and so on, without end. It is clear that on a moving-picture film the impression of temporal movement is caused not only by the consecutive *arrangement* of still "shots" (to which is owing also the illusion of spatial movement, if any) but also by the *actual movement* of the film. If the reel of film symbolizes time, the movement of the reel may symbolize another dimension of time.

Now let each of us imagine that he is an actor in a moving picture and that, having obtained a print of the film, he is projecting it. For simplicity, let us assume that nothing has been deleted that was photographed and that the scenes were taken in order without interruption. The man beside the projection machine may, if he wishes, stop the film, skip a section, run it backwards, or see it from beginning to end; or he may spread the film out. In him we have an analogue to what Dunne designates as an observer in the fifth dimension, or more precisely, as a fourth-dimensional observer. To be perfect, the analogy would have to allow the watcher to relive the experience of making the picture. What does this signify? Dunne argues that each of us exists as an observer in every time-dimension of an infinite series. In ordinary time, the fourth dimension, each feels time as movement. In the fifth dimension, which times the fourth, each may see ordinary time as simultaneity if he cares to do so. So to speak, the "film" is always there for examination; and when he projects it he enjoys a double consciousness, as actor and as audience, experiencing and observing all of it at once or else concentrating on a single favorite scene. The other actors, the other members of the audience, are also there in every dimension, but they have only their own experiences before them;

if the Relativitists are right, they have them in moments adjacent to rather than identical with those of their neighbors.

Dunne contends that our nature as multidimensional observers may be verified by experiment. The projection machine, to continue the analogy, is operative in dreams. When we are awake we are captives of our attention-focus on the apparent present. When asleep, we are mentally freed from local distractions and can rove at will over the real present, namely, the fourth-dimensional "now," which we call our life-span. It should follow, and seems true, that dreams may report composite elements derived from separate events and, moreover, that they may hold up to the dreamer's consciousness real events in what to his waking self is *future* time. Dunne's investigations of dreams, including his own of which he had kept account, showed him that precognitive dreaming was even more commonplace than he had suspected and that it remained unnoticed because people tend to forget soon after they wake up.

Dunne did not stop with conventional dreams. One corollary from serial time is that the observer in the fifth dimension, or in any superior dimension, is immortal. With the presumed expiration at physical death of the attention-focus that limits the traveler along the fourth dimension, such an observer, as in the dreams of sleep, prolongs his dreaming. Or rather, he re-experiences the eternal present. He cannot, to be sure, engage in time-travel outside the strip of the fourth dimension bounded by the dates on his tombstone. Nor can he alter the immutable reality of his life. But he can choose his focus, just as in the first four dimensions one may choose to listen to Bach or to read Jane Austen or to drink Scotch whiskey—or to do all three at the same time. The unique thing about Dunne's immortality scheme, which he much elaborated in *The New Immortality,* is that it dispenses with the supernatural. From the spiritualistic point of view, it is therefore defective; for though it explains many psychical phenomena, it neglects phantasms.

J. B. Priestley tells us in *Midnight on the Desert,* published in 1937, that he read *An Experiment with Time* when it appeared in 1927. But not until 1935 did he reflect on the literary possibilities of serialism. He came back to Dunne in that year after reading *A New Model of the Universe,* by P. D. Ouspensky, the Rus-

sian mathematician and occultist, some of whose writings on time antedated those of Dunne by several decades, though not available in English until 1931. Ouspensky, in contrast with Dunne, posited only six dimensions: three of space and three of time. His fifth dimension holds the eternal duration of each moment of the fourth, and his sixth dimension holds the actualization of every potentiality not fulfilled elsewhere.

Now in 1932 Priestley had written his first play, *Dangerous Corner,* which, as he has noted, makes "use of the device of splitting time in two, thus showing what *might* have happened," an idea long fascinating to him. The method he adopted of manipulating this device was the old one of the "Chinese-boxes" plot. At the start of Act I, the principal characters are listening to the concluding dialogue of a broadcast melodrama. One of them switches off the set, and their own stage action proceeds. As this approaches its end, it seems to be a re-enactment of the radio play, down to the same catastrophe of violence. But, preventing such a *dénouement,* Priestley had them interrupt the action and return to the moment of silencing the radio in Act I. That is, he gave them another chance, the essence of which is that this time they will avoid a certain "dangerous corner"—an incident that has made their conversation a quarrel. The trick recalls Michael Redgrave's terrifying nightmare-within-a-nightmare film *Dead of Night,* produced a few years later, but is different in that it lets a single cause bring about alternative actualities.

Priestley's *Dangerous Corner* heralded his discovery of Ouspensky's recurrence doctrine. Ouspensky, after working out his system of the six-dimensional universe, plunged into some singular speculations to the effect that men and women, when they die, are reborn exactly as before; or, as he described it, the calendar and the clock are put back to the day and hour of their birth, and they live the same lives repeatedly. But each life may vary slightly from the others, so that, as in Oriental metempsychosis, a moral improvement or deterioration is possible. Not content with this notion, Ouspensky outlined one even more curious. When after many trials of the same lifetime a man has attained wisdom, he may be reincarnated as a different self, but not into the future. He may be reincarnated

into the past, the segment of history that needs reforming, and there he may labor to change the intervening time for the benefit of his own later contemporaries.

Although Priestley did not accept Ouspensky's recurrence and reincarnation, he wrote a recurrence play exploring the implications of some of this. The title, *I Have Been Here Before*, he borrowed from D. G. Rossetti's "Sudden Light," a poetic meditation on the experience called *déjà vu*. *I Have Been Here Before* was staged with excellent success in 1937. In it a man who has the faculty of remembering the future events of his life, which Ouspensky would say he had passed through in earlier existences, searches out some people who are about to come to grief and for the first time manages to avert their tragedy.

Priestley in *Time and the Conways*, also produced in 1937, drew heavily on J. W. Dunne. As ingenious as this drama is, it hardly makes as much dramatic sense as *I Have Been Here Before*. Priestley himself prefers it, perhaps because, in his own words, "the theory of Time found in this play . . . comes nearest to [his] own belief. . . ." But it must be a hard play to catch the drift of in the theater. It is about a family, the Conways, who in Act I are assembled at a party for the twenty-first birthday of Mrs. Conway's daughter Kay. Act II is a forward projection in time to Kay's fortieth birthday. Act III returns to the original party. Now Priestley has explained that the "whole point and quality" of *Time and the Conways* "are contained in the third act, where we know so much more about the characters than they know themselves." This is quite true: we know in Act III that the expectations of the characters are not to be fulfilled as they hope, that what promises joy will bring sorrow, and the like. We have seen the fulfilment in Act II in the time that for them has not yet arrived. Yet in Act III Priestley did not obviously enough underscore the irony, or incongruity, call it what you will, of the distinction between the desired and the actual future. As for Dunne's serial hypothesis, Priestley did not require it. Taking liberties with the time-order is within a playwright's power without such justification. He intended, however, to suggest that Act II showed the future in the fourth dimension as envisioned by an observer in the fifth,

namely the girl Kay, who at the close of Act I drowses off into a dream of the simultaneous future, nineteen years later. His purpose was to give Kay in Act III, after she awakes, a special insight into the problems of her family through her obscure recollection of the future that she has seen.

In 1938 came the play *Music at Night* (inadvertently repeating a title already used by Aldous Huxley), and in 1939 *Johnson over Jordan*. In both, Priestley availed himself of the serial observer, the observer in the fifth dimension, which is the remotest that one can have any steady view of as a reader, let alone as a playgoer. Priestley confesses to being perplexed and puzzled by Dunne's infinite regress of dimensions and observers; but no practical application of the hypothesis obliges one to worry about more than five dimensions. Yet for the stage even five may be too many. *Music at Night* depicts a group of people who during a private musicale become half-hypnotized into reliving the crises of their past. Scene by scene they stand forth to converse or to soliloquize in *Strange Interlude* fashion as each struggles with the meaning of dead time brought back by music. Priestley was attempting to point up the human resemblances, the community, beneath their surface disharmony. He had taken issue with Dunne's pronouncement, that in the higher dimensions personalities are more distinct, and was trying to dramatize a blurring of differentiation as the observers recede from fourth-dimensional separateness into identity. He has admitted that the play fails to carry the burden of this idea, but the work is technically as good as anything else he has done. *Johnson over Jordan* is simpler. Although it is the only play of Priestley's that invades the fourth-dimensional content of a *dead* observer's mind, the observations would be the same whether dead or living. Priestley's technical problems were not new, and he only had to find artifices to make an audience accept the situation. The shifts of focus from the mourning survivors to the dreaming dead man and back again are admirable. The melding in Johnson's observations enabled Priestley, as he presented in scenes the content of Johnson's world-line or fourth dimension, to create symbolic incidents conveying the moral message of the drama. Dunne never paid much heed to moral questions; to him, one observer

was like another. One would in fact suppose that Dunne's whole scheme could encourage only a calculating hedonism. But Priestley, preoccupied with man's ethical growth, required his Johnson to learn from his simultaneous vision of his life a lesson of control, the effect of which is to release him a wiser man into some more distant dimension where all observers blend with the divine ground. Dunne acknowledge the divine ground too, but he did not think that the Diety was interested in human redemption. *Johnson over Jordan* is Priestley's most nearly mystical work.

The most astonishing of Priestley's dramatic pieces is *The Long Mirror,* produced rather quietly in 1940. It exemplifies his later tendency to depart from the strict conditions imposed by Dunne upon what an observer may observe; for, it must be remembered, the fifth dimension restores but does not enlarge the experiences of the first four. But in *The Long Mirror* there is a case of telepathic eavesdropping. Instead of being limited to personal experience, the observer, a young woman named Branwen Elder, is able to penetrate into the consciousness of another person, whom she has never met. This is a certain composer, Michael Camber, who, it turns out, has had intimations of what she is doing. Her activities take the form of dreaming about Camber's thoughts, his telephone conversations, and his arguments with his wife, and, what is more, of manifesting herself to him, somewhat in the manner of the goddess to Endymion. The possibility is rather indecent (one wonders what Mr. Faulkner would say), and it does not seem calculated to induce between two such people the romantic attachment contrived by Priestley in bringing them together. But Priestley took the situation from real life; the case would be interesting to parapsychologists. The "long mirror" of the title is fourth-dimensional time; if it reflects images from all minds, it might perhaps disclose to any mind those emanating from another.

Priestley has written a few other serious plays and several comedies that do not depend on dimensional time. As an author of time-fiction in novels and tales, he is less adept than in the drama. He has always been a sentimentalist, from *The Good Companions* onward. But the time-stories collected in 1953 under the general title *The Other Place,* together with the novels *Jenny Villiers*

(1947) and *The Magicians* (1954), all of which have aroused interest in England, are not only more sentimental than the plays, they are less realistic and lack compensating charm. They may also have some moral imbalance in their didacticism. The time-plays exploit Dunne's or Ouspensky's hypotheses to reveal characters awaking to the majesty of a world of eternal reality and recognizing their potential delight, through responsibility, in a meaningful existence. Certain of the plays show personages oppressed with the futility of life in an ever-decaying world of matter, disappointed, frustrated, driven to neurosis if not to the verge of suicide; it is these people to whom the vision of enduring time affords solace. Priestley has declared, thinking of serial time, "We cannot be sent to heaven or hell, but perhaps we can build them for ourselves."

What better moral could or should be inferred from J. W. Dunne's serial universe or indeed from any universe conceived as arresting the fugitive current of time? The same moral pervades Priestley's stories and novels of the fourth dimension. But in these, one fears, instead of introducing the multidimensional world as a stimulus to constructive action within the limits of routine existence, he has let it become also a fairyland for morbid escape. The stricture is especially relevant to stories in *The Other Place*. The title narrative concerns a man who, as a result of a kindness to an elderly gentleman, a sort of empirical Dunne, is treated by him to a visionary excursion through a door in a wall into the Avalon of his own heart's desire. Taking into his hands a polished stone and staring fixedly into it for a few minutes, he becomes an observer in the fifth dimension and enters through trance into "a land where it is always afternoon," where there are food and drink and companionship and a beautiful woman. But, like the prince in Hans Andersen's tale of the Garden of Paradise, he makes a blunder and is sent back. Priestley shows that the quality of such an experience depends on the unselfishness of the observer. The moral is good, but the psychiatric implications are troublesome. The man who comes back from fairyland is always haggard and woebegone, certainly, and yet his fairyland ought to *be* fairyland, not a mental state into which he elected to withdraw. One is always uneasy when confronted with an empirical quest into the hidden

realms of consciouness. Two images rise: the saint ecstatic in the rapture of the divine embrace; the catatonic schizophrene lost forever in who knows what hell of vision. One shrinks from playing a game with hypnotism without knowing which of these two states one might encounter.

The two novels, *Jenny Villiers* and *The Magicians,* are each about the reclamation of a despondent middle-aged man from his wasteland of three vanishing dimensions. In *Jenny Villiers* a worn-out playwright, becoming an observer in the fifth dimension through an overdose of cardiac pills, learns that the past is eternal and from his vision comes refreshed to continue his created art. This novel would be better if one could believe this man consecrated and not in some preposterous way brain-washed. The defect lies perhaps in the unreligious content of the experience, presided over only by the young actress Jenny, to whose life of a hundred years before the dreaming playwright gains access. *Jenny Villiers,* in contrast with Robert Nathan's pathetic little romance *Portrait of Jenny,* published seven years earlier, is not properly a ghost story. It is a narrative of time-travel, indebted one may suppose to Anne Moberly and Eleanor Jourdain's *An Adventure* (1911), their classic account of having crossed a loop in time to the eighteenth century at Versailles. It is also a narrative of telepathic eavesdropping, reminiscent of *The Long Mirror.* Priestley's latest novel, *The Magicians,* takes from Ouspensky, it seems, a hint for the characters from whom this book gets its name. The three wise men who bring to the disheartened Sir Charles Ravenstreet a means of rehabilitating his life, by causing him to see that dead time is alive and by helping him to live again a forgotten espisode in his career, behave like men reincarnated out of the future in order to amend the past. But Priestley does not say what they are. He only lets his magicians talk about changing the past and evinces pleasure, as anyone might, in the thought of that miracle.

Priestley has indulged overmuch in fantasies, not necessarily with regard to Dunne and serial time. Dunne, in contradistinction to Ouspensky, was a quite orderly philosopher. If he is little read, the fact is owing in part to his difficulty, though this is not excessive, and in part to the seeming uselessness of his system, not to

say its implausibility. That it might be useful is clear from the moral and humanitarian counsels Priestley bases upon it. But in these there may lie perils. Any complex, home-wrought ethic generates the risk of superstition. Priestley, like Dunne, is balanced and reputable, but the fictional characters that spread his gospel are quacks and gulls. Set forth imaginatively, such a philosophy as Dunne's may sustain on occasion a thoughtful play, an instructive tale, but as frequently, it may but multiply the eccentricities of literature. There is much practical wisdom in the lines from Eliot's *The Dry Salvages,*

> Men's curiosity searches past and future
> And clings to that dimension. But to apprehend
> The point of intersection of the timeless
> With time, is an occupation for the saint. . . .

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SUCCESS WITH COMPLEX TARGETS IN A PK BASEBALL GAME

By Douglas Steen

ABSTRACT: This report describes the results of a gamelike experiment called "PK baseball" in which three dice (one red and two white) were rolled on each trial, and success or failure was determined by relating the face combinations to the rules of baseball. The face which turned up on the red die indicated that the baseball was hit onto the playing field, that the umpire called a strike, or that the umpire called a ball. The combinations of faces on the two white dice were scored either as base hits or as fielded outs. The white dice combinations counted in the game only when the red die indicated a ball hit to the playing field, and these are called "actual" hits and outs. All white dice combinations which occurred when the red die indicated a called strike or ball are called "latent" hits and outs.

The experiment consisted of 100 PK baseball games played between one subject, A, and the experimenter; and 14 games between subject B and the experimenter. The results were evaluated in three main ways, none of which involved any assumptions regarding the trueness of the dice. First, there was found to be a significantly higher proportion of actual hits to actual misses than of latent hits to latent misses; that is, hit combinations tended to occur on the two white dice when they actually counted in the game. Secondly, there was a significant decrease in the proportion of both actual and latent hits when the first 40 games were compared with the last 40. Finally, there was a significant decline from the first 40 to the last 40 games in the proportion of times the ball was knocked (according to the red die) into the playing field.

The results are presented as a first step in the investigation of the question of how complex a target situation PK can influence to a measurable degree.

Mr. Steen is a physicist engaged in business. Although this is his first report for the Journal he has long been actively interested in the research.—Ed.

Introduction

Is PK capable of influencing several target objects simultaneously so as even to control their relations to one another? This paper describes exploratory experiments which were conducted as a first step toward answering this question.

The kind of complicated task to be studied was not simply one in which a number of dice were thrown together for target face or placement. Rather, the interest in the present research centered upon the question whether PK would be able to influence the combination of faces, but not for a mere sum as in the old high-dice and low-dice tests.

As a means of contributing to the realism of the task as well as fostering the lively interest and strong motivation essential for PK success, a game of "PK baseball" was invented. This provided a means of scoring combinations of the dice in terms of the plays and rules of the real game. The writer hoped that the game would provide a situation of sustained favorable motivation which would prevent the occurrence of declines in PK success. However, it will be apparent that the results did not bear out this hope.

The Game

Baseball was chosen as the game to be played because it is widely known and to many people it is *the* national sport. More important, it was a game in which the prospective subjects were already interested and it was felt that their personal involvement in this sport would lead to the development of the strong motivation required in the PK test. Similar PK games with dice can be worked out for other sports, and I have already invented a game for "PK football." The availability of different games would enable the experimenter to select the task to suit the subjects' preferences and also to take advantage of seasonal interest in sports. Games of varying degrees of complexity would lend themselves to a more extensive investigation of the manner and degree of PK success with different complex tasks.

PK baseball is played with three dice, one colored and two white (or the reverse). Each throw of the three dice results in a ball, a strike, or a hit of the ball to the playing field (for a hit or an out). Which of these three possibilities is realized depends upon which face of the single (colored) die is up, as shown in the following chart:

Colored Die	Result for Player at Bat
1 or 2	Ball is hit to the playing field
3 or 4	Umpire calls a ball
5 or 6	Umpire calls a strike

When the colored die is 1 or 2, reference is made to the white dice to determine further details of that particular play. When the bat has connected with the baseball and driven it to the playing

field (1 or 2 on the colored die) the player's success is scored according to the following combinations on the white dice:

White Dice Faces	Effect Upon Play
44	Home run
33	Triple
22	Double (advances other runners 2 bases)
54 or 11	"Sharp" single (advances other runners 2 bases)
52 or 32	"Soft" single (advances other runners 1 base)
Other combinations	Batter is out and runners hold

The game proceeds by innings just as in the outdoor sport. The result of any cast of the dice, in terms of the baseball game, can be spotted at a glance after a slight familiarity with the rules. The experimenter may help to build up and maintain the atmosphere of competition by following the game in the manner of a radio announcer.

All three die faces are recorded on each cast, the single die first in each instance. When, as in the present experiment, only the two people who take turns at bat are present, the one records while the other has his inning. However, the player at bat should always be responsible for observing the accuracy of the recording.

Procedure

This research was carried out at the writer's home in Los Angeles. It consisted of four sub-experiments: (1) From November, 1949, through January, 1950, 40 games of PK baseball were played between the experimenter and subject *A*. (2) From February, 1950, through June, 1950, 20 games were played between the experimenter and subject *A*. (3) During this same period, 14 games were played between the experimenter and subject *B*. (4) Finally from July, 1950, through December, 1950, 40 games were played between the experimenter and subject *A*.

The contestants sat on opposite sides of a large wooden rectangular playing surface. The dice used were of the ordinary commercial, inexpensive variety with excavated spots. A small pool of the red

and white dice was available throughout the research period and for each game the three dice required were selected at random from this stock. Before each throw, the three dice were shaken for about ten seconds in a glass. They were then rolled along the table to a backboard. Occasionally one or more of the dice bounced off the surface and fell to the floor, and they were scored as they lay. The dice were picked up after each throw with the same faces uppermost and were returned to the glass to be shaken for the next cast.

The object of baseball is to score runs and defeat one's opponent. The best way to do this is by registering hits, particularly extra-base hits. One will naturally have a strong desire to get hits, and also a desire to earn a walk on balls and to avoid getting out on strikes. Hence, we assume that target success consists mainly of hits and, secondarily, of balls. Target failures consist mainly of outs made when the baseball has been hit to the playing field, and secondarily of strikes.

Immediately after each game the data were scored and checked. Furthermore, when the data pages were tabulated at a later stage, the summations were cross-checked. By this method errors in checking were revealed and eliminated.

A typical record of a half inning of play is shown below. The first figure in each group of three indicates the uppermost face of the red die and the other two the combination for the white dice.

552 (Strike one)	
522 (Strike two)	
321 (Ball one)	
653 (Strike three; out)	No runs
463 (Ball one)	1 hit
152 (Hit)	
564 (Strike one)	
231 (Out two)	
142 (Out three)	

In the half inning shown, the ball was hit to the playing field three times, one of which was a hit (target success) and the other two, fielded outs (target failures). On the other casts of the dice,

the red die scored either a strike or ball and the results on the white dice were, therefore, only "latent" hits or outs. In two instances they gave latent hits and in four others latent outs. One strike-out occurred and the other two outs, which retired the side, were made in the field.

Results

To avoid making assumptions as to the physical structure of the dice, I have used methods of analysis and evaluation which compare data with the same dice under different conditions. This approach will not evaluate hits, walks, etc., but will measure the difference in results with the white dice. The primary facet of the analysis will be the relative number and distribution of hits among data for the white dice when a hit to the field is indicated by the red die and the same white dice when a ball or strike is indicated. The first are called actual (non-latent) hits and the second, latent.

Table 1 includes *all* the data on the white dice combinations.

Table 1

Comparison of Ratio of Actual Hits and Outs with Ratio of Latent Hits and Outs

	Actual Hits	Latent Hits	Total
Hits	2016	4104	6120
Outs	5032	11070	16102
Total	7048	15174	22222
Percentage of Hits	28.6	27.0	

$x^2 = 5.85$ (1 d.f.)
$P = .016$

The red die results are considered in the table, but only as the basis for designating the white dice combinations in the categories of actual and latent hits or outs. The white dice results yielded 28.6 percent hits in the actual hits category and 27 percent hits when the red die rendered them latent. A chi-square test applied to these data

indicates that the proportion of actual hits is greater than that of latent hits to a degree that would be expected in random sampling only 16 times in a thousand (P=.016). The result is marginally significant.

This over-all analysis of the results thus gave reasonable assurance of the operation of PK in the experiment as a whole. The supposition is that the subject favored hit combinations more effectively when the red die made a hit to the field. It therefore seemed worth while to conduct further exploratory analyses to see what light might be thrown upon the way in which the process operated and the way the effect was distributed. In these further analyses, attention was focused primarily upon the data of those games in which subject *A* and the experimenter participated. Because of the small number of trials conducted with subject *B*, the results of the games with him did not lend themselves to the same kind of analyses.

Further study was made of the chronological distribution of actual and latent hits throughout the experiment. The percentage of hits was obtained both for the latent hits and for the actual hits for the three main chronological sections of the experiment when *A* was the subject: the first 40 games, the middle sub-experiment of 20 games, and the final 40 games. As may be seen in Figure 1, there was a decline throughout the experiment in the percentage of hits with the two categories combined, and this decline occurred at approximately the same rate for the actual and latent hits separately represented. As a test of the significance of the decline, Table 2 shows an analysis of the difference between the first and last sub-experiments. The ratio of the pooled actual-and-latent hits to the pooled actual-and-latent outs is shown for each of these groups of games. The percentage of total hits in the first 40 games was 28.7; and in the last 40, 26.6. The chi-square for the difference between the two ratios is 8.65 (1 d.f.) with P=.003. Thus there was a decline in the general success of both players in producing the white dice combinations which scored hits.

A more detailed picture of the chronological decline is shown in Figure 2, where the percentage of actual and latent hits for both players combined is given for each successive ten-game group.

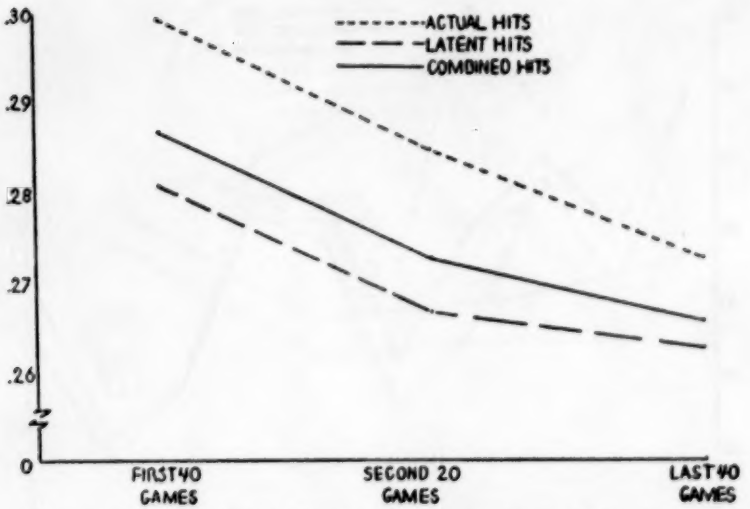

FIG. 1. Chronological decline in percentage of actual hits, latent hits, and the two combined for both *A* and the experimenter.

Table 2

COMPARISON OF THE COMBINED NUMBER OF ACTUAL AND LATENT HITS AND OUTS IN THE FIRST 40 GAMES AND THE LAST 40 GAMES WITH SUBJECT A AND EXPERIMENTER

	First 40 Games	Last 40 Games	Total
Actual and Latent Hits	2239	2104	4343
Actual and Latent Outs	5557	5800	11357
Total	7796	7904	15700
Percentage of Hits	28.7	26.6	

$\chi^2 = 8.65$ (1 d.f.)
$P = .003$

While Figure 2 in general reflects the same over-all decline as shown in Figure 1, it suggests that the decline was not a regular one. There is a striking U-shaped distribution of hits in both categories

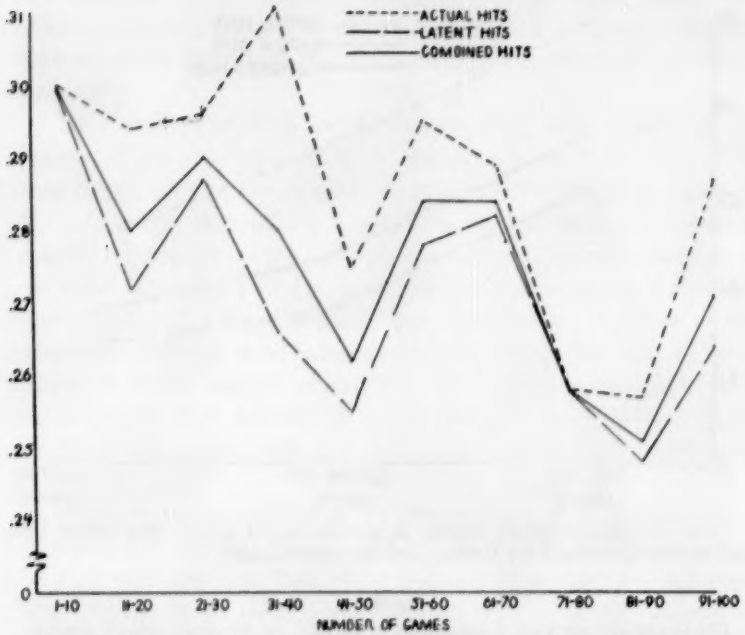

FIG. 2. Chronological decline in percentages of actual, latent, and combined hits for each successive ten-game group for subject *A* and experimenter.

within each sub-experiment. As a means of representing this distribution, the data of each of the three sub-experiments were divided into chronological quarters and the corresponding quarters were pooled. This distribution of hits within the pooled sub-experiments is given in Figure 3. It shows the characteristic U-curve of beginning and ending salience, especially in the curve for the actual hits. The CR of the difference between the results of the first and fourth quarters combined and the second and third quarters combined is 1.31 (P=0.19).

The distributions of actual and latent hits by ten-game segments for subject *A* and the experimenter individually are shown in Figure 4. There is an apparent tendency for the curves of the two subjects to agree closely during the first half of the experiments and to diverge during the second half. It is interesting to see that the ex-

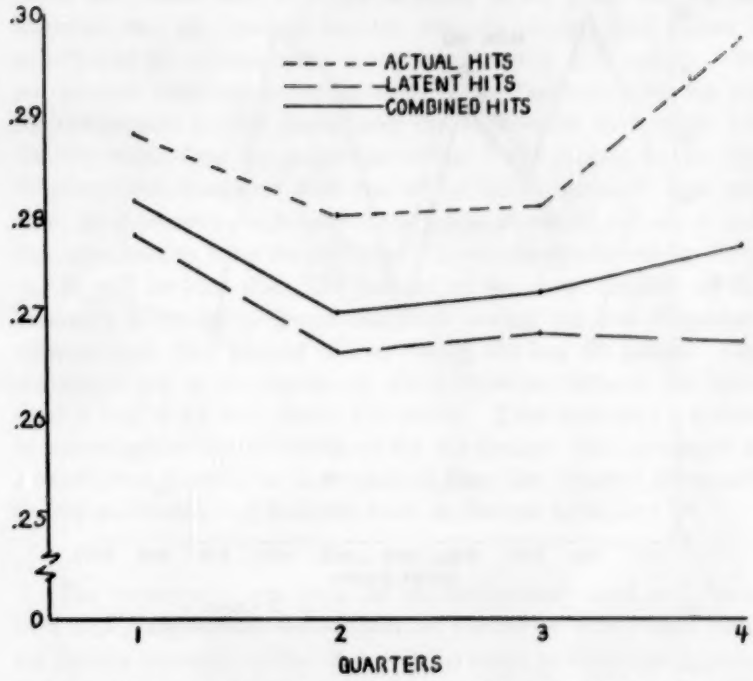

FIG. 3. Data for three sub-experiments pooled and divided into chronological quarters for subject *A* and experimenter.

perimenter obtained the higher percentage of actual hits during the second half of the experiment, while subject *A* obtained a higher percentage of latent hits. This fact, together with the shift in the relative success of the two players between the two halves of the experiment, gives further assurance that the non-chance effects in the data did not arise through faulty observation, imperfect dice, or other artifacts. This is particularly true in view of the fact that within any given game both players always used the same dice.

Thus far the analyses have been concerned only with what happened to the two white dice when they were separated, on the basis of the red die, into actual and latent baseball values. This leaves for separate consideration the examination of scoring trends with regard to the results on the red die itself. For information on this

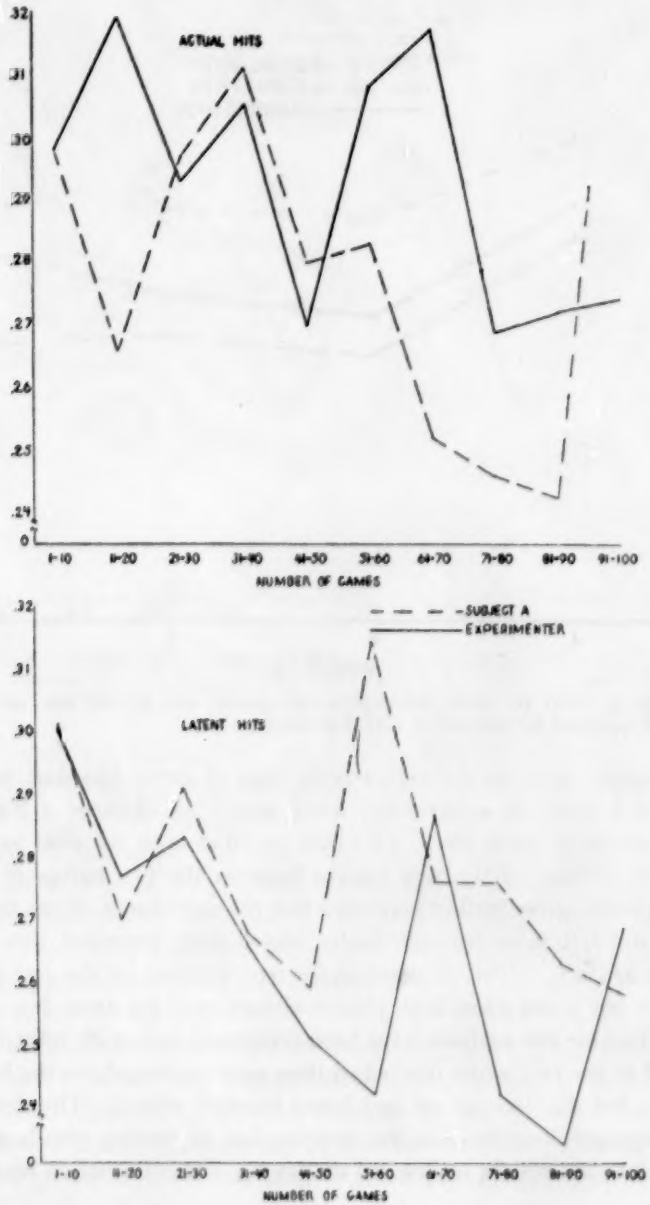

FIG. 4. Distributions of actual and latent hits by ten-game segments for subject *A* and experimenter separately.

point, the results were analyzed in terms of the times the red die indicated that the baseball was hit into the playing field (faces 1 and 2) and those times when it was not (faces 3, 4, 5, and 6). To see whether there was a decline effect in the "success" with the red die comparable to that found over the experiment as a whole for the two white dice, the proportion of the 1 and 2 faces in the first 40 games was compared with that of the last 40 games. This was done, as previously, with only those games in which subject *A* and the experimenter were the players. The results are shown in Table 3. It will be seen that 32.9 percent of the casts resulted in the baseball's being hit to the playing field during the first 40 games, whereas only 29.7 percent did so during the last 40 games. The chi-square test of the figures on which these percentages are based yield $X^2=17.8$ (1. d.f) with a P=.00002. Thus there *was* a decline in achieving the desired results on the red die over the experiment as a whole, and it was even more marked than that reported previously for the combination of desirable faces on the two white dice.[1]

Discussion

The experiment was done on the exploratory level and therefore was designed for the purpose of indicating worthwhile areas for further research, rather than as an attempt to reach conclusions. Even so, the value of the research depends upon whether the data provide reasonably strong evidence that PK was operating in the experiment. Statistically significant results were found in three principal analyses: (1) the higher proportion of actual hits as

[1]We are indebted to our two statistical editors for pointing out that, because of the experimental procedure employed, the evaluations made in Tables 2 and 3 can only be considered as approximations. As stated in the paper, the dice to be used in a game were selected from a pool of common red and white ones. In Tables 2 and 3 (but not in Table 1) comparisons are made of the frequency of occurrence of designated face combinations in the first and last parts of the experiment. This does not assume trueness in the dice; but it does make the assumption, which can be questioned, that the sampling procedure was adequate to randomize and equalize the effects of dice bias. One of the statistical editors suggested an alternative analysis. However, in view of the facts that the research was exploratory and the primary purpose of this report is to introduce the PK game as a research instrument, it seems advisable to publish it without further delay. The basic analysis comparing actual and latent hits is not open to criticism on the score of possible dice bias, and the consistency of the results from the white and red dice shown in the other analyses (both of them showing declines from the first to the last parts of the experiment) gives some reassurance that the statistical significance of Tables 2 and 3 is not in serious error.—Ed.

Table 3

COMPARISON OF PROPORTIONS OF BALLS HIT TO THE PLAYING FIELD IN FIRST 40 GAMES AND LAST 40 GAMES WITH SUBJECT A AND EXPERIMENTER COMBINED

	First 40 Games	Last 40 Games	Total
Ball Hit to Playing Field	2563	2351	4914
Ball not Hit to Playing Field	5233	5553	10786
Total	7796	7904	15700
Percentage of Balls Hit to Playing Field	32.9	29.7	

$x^2 = 17.8$ (1 d.f.)
$P = .00002$

compared with that of latent hits; (2) the chronological decline of actual and latent hits during the experiment; and (3) the chronological decline in the proportion of balls hit to the playing field. These analyses were applied to the data because they involved no assumptions regarding the trueness of the dice. The question of physical bias of the dice thus does not arise.

As an exploratory experiment, the conditions did not provide the most rigorous safeguards against recording error, but the research offers adequate reassurance on this point. All the recording was carefully witnessed by two players. Furthermore, the data did not turn out as one would expect if there were *motivated* errors in recording, and these are the only kind that could introduce any consistent misleading trend. In the first place, each player recorded the results while the other was at bat. Since the motivation of the game—the desire to score more runs—was predominant, and since each player recorded for the other, motivated errors of observation would most likely have reduced the number of actual hits for both subject *A* and the experimenter.

Another feature of the findings was the decline in results (both in the times the ball was hit to the playing field and in the number of

hits) from the first to the last sub-experiment. I did not expect a decline, since one of the purposes of introducing the game was to avoid this effect by keeping the motivation high.

The analyses of the data involved cross-tabulations which provided good assurance against checking errors.

The only question remaining is whether there was unwarranted selection of the most favorable analyses. The only analyses considered were those which made no assumptions regarding the trueness of the dice, and therefore I was limited from the beginning in the number of ways to examine the data. For example, the *difference* between players might have been made a point of emphasis. But the fact that three *independent* measures of significance yielded probability values of .016, .003, and .00002 makes it unlikely that any amount of correction required would reduce the over-all results to the chance level. It seems fully warrented, therefore, to accept the PK interpretation of the results and to consider some of the implications for psi.

The target provided by the use of the one red and two white dice interpreted in terms of the rules of the game is more complex than any previously reported for successful PK tests. There have been experiments in which the target was a combination of two dice (the tests for high- and low-dice, sevens, and doubles), but in these the subject was attempting merely to get a desired sum or combination of the two faces. The target in the present experiment was more devious. Whether or not a particular combination on the two white dice was a target success or failure depended upon what happened on the red die. The results cannot be explained, therefore, on any assumption of a general PK influence acting upon all three dice in the same way (such as that the subject was trying for "low-dice," since the low faces were desirable on both the red and white dice). Rather, the only hypothesis that seems to apply is one which recognizes that PK was acting in a way that took account of the relation between the white dice and the red one. This experiment, therefore, is a further step toward finding out what degree of complication of target is possible for the PK process. The question is obviously one that needs to be carried further, as

it is a line of inquiry that might throw considerable light upon the broad and basic problem of the limits of the PK function with respect to the physical world.

Although the fact that the tests were in the form of a game did not prevent a general decline in results, the procedure was a valuable addition to the test conditions. The game did hold the interest of the player over the extended series, and it is quite likely that similar results could not have been obtained by us if we had been trying merely for dice combinations without this framework. Undoubtedly, also, the game provided the chief motivation of the moment during each session, and this had the effect of keeping the subjects from becoming overly concerned or self-conscious about their PK score. This is, I believe, the first time that a PK test has been put within such a framework and the success achieved in this instance amply warrants further efforts along this and similar lines. It will be especially interesting to know whether other successful players of PK baseball will find the same effects in their results as those reported in this paper. The work done with games thus far in both ESP (1) and PK tests indicates that they have a certain value (doubtless more than has yet been realized), but that they provide no magical guarantee of success. The game may help toward achieving essential psychological conditions, but it will not automatically provide them.

Conclusion

It is not possible to explain these experimental results in terms of a single PK goal, such as low-dice. The differences and consistencies between actual and latent scoring rates appear to be indicative of individual die influences for the production of complex target combinations.

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EDITORIAL

ORDER IN THE FINDINGS OF PARAPSYCHOLOGY

In appraising the discoveries of a new field such as parapsychology the cautious scholar will probably be most influenced by the degree of lawfulness he observes in the results. It will doubtless be evidence of system, of regular recurrence of the same properties and relations—in a word, of *order*—that, more than anything else, will afford the confidence required for acceptance. Only if and when the mind finds it possible to organize novel results into a rational system of relations will the new research field "make sense" for the critical inquirer. Other aspects, of course, are important, but the significance of a body of logically consistent and rationally integrated findings surpasses all other considerations entering into the final judgment.

The importance of an orderly aspect in a branch of inquiry is especially great when the bearing of the field is as revolutionary as that of parapsychology. The radical bearing of results in this field means, of course, that the findings are out of line with currently conventional science. They challenge an assumption about the existing order of nature as conventionally regarded; and if the new set of facts is to be accepted, a significant revision will be necessary to accommodate them. This upsetting effect of a new upon an older order makes it all the more necessary that discoveries like those in parapsychology show recurrent regularity and system.

That the findings of parapsychology are developing into a well-organized network of lawful relations is now apparent to those who have kept pace with the progress of the research, although naturally this order is not easy to appreciate by those who have only a limited acquaintance with the results. This new branch of investigation may, in fact, be favorably compared (especially if its brief history and limited personnel are duly considered) with some of the more conventional problem-areas of psychology such as hypnosis, psychoanalysis, or even the larger division of psychopathology itself. Generally speaking, progress toward an experimentally established and rationally organized system of findings in the field of psi in-

vestigation stands up well against any comparable branch of purely psychological (e.g., excluding physiological) pioneering of the century. For those interested in appraising parapsychology as a beginning science the aspect of order is, indeed, worthy of examination. In it may be found the best index to the proper scientific progress of the field.

* * * * * * *

The first point of rational assurance about a new branch of inquiry concerns the method of approach. It is always a great aid to judgment as to the validity of a research if the investigations follow a familiar road to truth—one already traveled by the older sciences. This the research in parapsychology undoubtedly has done. It began, as have so many of the branches of natural science, with observations of spontaneous happenings, experiences that raised questions and led to the suggestion of hypotheses to explain the experiences. These hypotheses were in some instances testable by experimental methods, especially the hypotheses of telepathy and clairvoyance, and the trial ideas which these terms came to represent were investigated until, in the course of time, crucial experiments were performed and the establishment of these types of extrasensory perception was achieved. Thus parapsychology has had a double approach to its results; first, the case-study method and, second, experimental confirmation. To find in the laboratory evidence of what was first observed in spontaneous nature always gives the findings a broader and more realistic basis than one approach alone.

A second aspect of order in the data of parapsychology emerged first in the spontaneous experiences as they were found to fall into four general types of phenomena. Three of these, telepathy, clairvoyance, and precognition, are of the cognitive type and are, of course, considered forms of extrasensory perception. The fourth is psychokinesis. In one collection of spontaneous cases after another the reported experiences were found to group naturally into these four types. It is an acceptance of the order of nature to classify these phenomena in this way, much the same as other events fall into natural species. Moreover, in the experimental studies themselves (on which, of course, the conclusions regarding the occurrence of

these phenomena must rest) the same four types are found as definitely isolated as in the spontaneous material. However superficial the distinctions among the four types may eventually turn out to be, beyond question there *are* these types of psi phenomena and they are as clear-cut in the experimental results as in the spontaneous. In this aspect their order is unmistakable.

Third in this review of points of consistency coming out of the psi researches might be listed the fact that all these types of psi occurrence transcend physical explanation. Again we can begin first with spontaneous psi experiences, and again we find that none of the four types can be accounted for on any physical theory. But the laboratory evidence, too, is equally consistent on this point; not one of the four types of psi has shown any significantly regular relation to the varying physical conditions over which the experimental work has ranged. Thus the four types of phenomena in both kinds of evidence agree in establishing the nonphysical nature of psi.

As a fourth indication of order in the psi findings let us consider one of the main psychological characteristics of psi and see how systematic it looks. When spontaneous psi phenomena were first scientifically studied and the attempt then made to test the earliest hypothesis suggested, clairvoyance, the experimenters assumed that the ability investigated would be to some extent *voluntary;* that is, that it could be made to function at least to a degree as the subject willed; for the voluntary effort of the subject to try to perform as instructed was plainly necessary if a successful test was to be made. The tests were designed on that supposition and it turned out to be correct. Though the performance was limited in many instances to a very slender trace, yet it was possible to make a successful demonstration. Such, however, is the consistency of nature of these parapsychical functions that all four types, whether of ESP or PK, showed the same property of being subject to voluntary control. Tests on all four succeeded in showing that the effect could be demonstrated to a significant degree upon a specified target existing in a given time and location and distinguished from all others.

A fifth aspect of lawfulness to be observed in the operation of psi is likewise a psychological one; namely, the *unconscious*

character of the essential psi function. This aspect was first recognized by workers in the laboratory phase of investigation, and again it held as truly for telepathy as for PK, for precognition as for clairvoyance. But when the question was carried back, as it was, to case analyses, an echoing confirmation was found there as well. There were differences, it is true, in cases of spontaneous psi occurrences, especially in instances of strong emotion or great significance. Often in these occurrences a clear conviction of the truth of the impression received was shown. On inquiry on the experimental side some slender occasional trace of conviction of accuracy was discernible. These impressions of confidence are not, however, to be mistaken for consciousness of the basic psi function itself. Rather, in all eight of the subdivisions of phenomena where an answer might be sought, the same peculiarly hidden character of the underlying process is indicated. In fact, the operation of psi seems to be the most irrecoverably unconscious of all the functions as yet identified by psychological research.

A full enumeration and recounting of all the consequences of the unconsciousness of psi that have shown consistency and lawful recurrence would require excessive space in this review. The mere fact, however, that these many effects which once seemed inexplicable can now be attributed to the unconsciousness of the essential operation enables us to regard them all as an integral part of a larger system.

One of the best known of these effects resulting from the unconsciousness of psi is known as *displacement*. In this occurrence the subject in a test hits the target adjacent to the one intended. Displacement has occurred in tests of all three types of ESP and an equivalent effect has been claimed even in some of the evidence for PK. Another recurrent result of the unconscious character of the psi function is the tendency of the subject in a series of tests to *decline* in scoring rate as he progresses down the column of responses or through a "set" of columns. This decline effect, too, is found in all four types of psi experiments.

One of the most often manifested of the psi occurrences springing from the unconscious nature of the ability is the subject's tendency under certain conditions to score significantly below the

chance average. It is what is now called *psi-missing*. It may indeed be that this tendency to avoid the target is not confined to psi but would hold just as well in other psychological processes if similar tests could be carried out under comparable conditions, as for instance in marginal sense perception or other delicate cognitive functions such as memory. Such tests have not been made, but we do know that under certain conditions this feature of missing may occur in *all four* types of the psi process. Naturally it adds a great deal to the impression of lawfulness in the field to find that so peculiar and unexpected an effect as psi-missing, encountered first in one type of psi, was also found later in the other three.

* * * * * *

How well does present knowledge of psi allow it to fit into the larger scientific picture of the order of nature? Except for the single characteristic that it requires something more than physical law to explain it, there is no important incongruency. One of the more reassuring features of the results of the psi investigations is the way the *distribution* of psi ability among the general population has continued to be found to extend far and wide, both as to spontaneous case reports and experimental subjects. It was inevitable that eventually the working hypothesis would emerge that psi is a capacity of the species, one that has become a function of the organism. This idea logically led to the question concerning an early evolutionary origin for psi capacity, which, in turn led to an investigation of psi in animals. A modest amount of evidence has already been obtained showing that it does occur to some extent in animals as well as men. Thus it may be seen that this organized extension of knowledge of the distribution of psi capacity follows the same sort of orderly lines familiar to scientists of all fields.

Other effects beyond the mere evidence of psi reflect this same spread of distribution by their recurrence from experiment to experiment. Examples could be multiplied of the discovery of similarity in patterns of distribution from one research to another, across gaps of considerable breadth, between researches conducted by different experimenters with different subjects and involving a considerable difference in conditions and procedure. For example, such psychological patterning has often shown up in the diagonal

declines in hit distribution (down and across the page) as revealed in the QD (quarter distribution) studies of PK test results. When this effect was found to preponderate in the eighteen series to which the analysis could be applied, a similar decline came out in the twelve of the eighteen series in which a smaller unit, the set, could be taken as the basis for a similar QD analysis. It continued even in the nine series to which the half-set analysis of the same type could be applied. The order that is stamped throughout these analyses is unmistakable and brings to the critical mind an assurance of a lawful order of a new type, an order that emerges from the distribution of hits as a test progresses. It has the bracing value of another dimension.

In general, then, psi operates like a natural process insofar as its properties are understood. In spite of its elusiveness it lends itself to quantitative measurement and experimental controls in much the same way (if not degree) as other psychological processes and indeed much better than some. Except for the handicap of being beyond conscious control, psi phenomena are subject to the normal motivation of the individual and are as purposive and voluntary as other aspects of mental life. They are influenced by a wide range of conditions, subjective and physiological, much the same as other delicate capacities of the personality. ESP and PK form a means of energetic interaction between subject and object that parallels the sensorimotor system itself, again with the difference only that the sensorimotor capacities operate on a known physical basis and ESP and PK are known *not* to be operating on such a basis. It is the energetic operation between subject and object that is different. To account for psi phenomena it is necessary to infer a nonphysical energetic influence in nature—as has often before been proposed to explain mental action. Now the need is experimentally confirmed.

Perhaps it is most impressive to many to learn that psi is a sufficiently orderly function that it can be demonstrated from one situation to another if enough care is given to see that the two situations are similar. And as far as the term "repeatable demonstration" can be applied to any psychological experiment, the tests, especially those conducted in recent years in the schools, have been

found to show orderly repeatability if the same conditions are reproduced.

In looking for the highest order manifested by the psi investigations we may well look beyond conventional science and ask where the field belongs with respect to the universe as men see it and live in it. Mankind lives under a concept of the order of nature that is far from being a scientific one. Other types of knowledge and belief than those of science make up this concept of the ordered world. For most of the peoples of the world this concept is preponderantly one in which a little science, a great deal of common sense, a little intuition, and varying degrees of religious doctrine are all woven together. Within this picture the concept of psi as an extraphysical aspect of human nature more readily finds an orderly place than does the picture of man derived from the conventional sciences themselves. Here for the first time is a finding of science that *does* support our cultural heritage of beliefs about man's nature.

When it is remembered that the whole value system of civilization has been built on the assumption of nonphysical or spiritual elements in human life, it can be appreciated that the effect of the psi investigations is to bring some order out of the existing chaos in which the modern intellectual finds the question of man's place in nature. In correcting the physicalistic interpretation of man which the pressure of modern science has long been thrusting upon a world of values it did not fit, the findings of parapsychology help to bring harmony into an area of the most vital importance. If order has significance anywhere in the human scene it must indeed be in this larger framework which gives meaning to modern life—on the individual, the national, and now the international scene. It is time, indeed, to find a tenable and factual order of nature adequate to resolve the differences—philosophical, ideological, political—that are today disordering human life and disrupting its peace of mind as never before was possible. What better—indeed what other—defense could there be against this confusion but a firmly tested structure of corrective knowledge of what men really are?

J. B. R.

LETTERS AND NOTES

NOTE ON CONSISTENT MISSING

By Remi J. Cadoret

Since its initial description seven years ago (1), consistent missing has received very little confirmation. The only known published description of a new example of this effect is mentioned by Nielsen (3) as an adventitious finding in a case of precognition. Just how much this lack of published confirmation reflects a true picture of the frequency of occurrence of consistent missing is questionable. When it was first found, it was hoped that it would furnish a clue about the nature of the ESP process. It may well be that its apparent infrequency will prove to be just as valuable a clue. The purpose of this note is to present evidence of consistent missing in some recently published data, indicate why it may have occurred in this situation, and show how it might aid in understanding ESP.

Fisk and West (2) reported a case of ESP which was remarkable in that the subject responded significantly above chance on two ESP symbols which had a strong sexual connotation for him. The subject claimed that the circle and cross symbolized the male and female genital organs respectively. The erotic nature of these two symbols was further emphasized by the experimenter in the course of testing by covering the circle and cross symbols in an ESP deck with appropriate pictures. In their report, Fisk and West published a four-place table to show that the subject seemed to recognize when the target was one of the two erotic symbols by hitting those symbols more often than chance would allow. From inspecting this table, it seemed that the large differences noted between success on the two erotic symbols and the other three ESP symbols were not totally accounted for by the direct hits and that consistent missing might be occurring. To test for this possibility I made up a five-by-five contingency table shown in Table 1 which gave a significant chi-square when evaluated for consistent missing. (See the Ap-

pendix of Reference 1 for method of evaluation.) Because this analysis was not planned before the data were collected, it is difficult to assess the results as conclusive evidence of consistent missing; it is mainly for their suggestive value that the results are presented here, since the associations seem so meaningful psychologically.

The reader can see from Table 1 that when the misses alone are considered, the subject seems to have responded to certain

Table 1*

Calls \ Cards	^	=	□	♀	♂
^	10 0 8.27	8 (−) .14 9.14	15 (+)5.88 8.10	4 (−) .65 5.97	3 (−)2.12 6.80
=	13 (+) .10 11.92	9 0 13.18	8 (−)1.15 11.67	7 (−) .30 8.60	14 (+)1.80 9.80
□	15 (+)4.99 8.49	5 (−)2.06 9.40	11 0 8.32	4 (−) .74 6.13	7 (+) .0006 6.98
♀	7 (−)1.03 10.24	16 (+)1.93 11.32	9 (−) .11 10.03	22 0 7.38	8 (−) .02 8.42
♂	3 (−)2.57 7.35	9 (+) .09 8.14	5 (−) .67 7.20	11 (+)6.10 5.30	17 0 6.10

$\Sigma\chi^2 = 32.45$ (11 d.f.)
$P \sim .001$

*Key to figure in each cell: upper right = observed frequency, lower left = expected frequency, sign in enter = sign of deviation, center = chi square. ♂ = male erotic pictures. ♀ = female erotic pictures.

neutral symbols by calling them another neutral symbol (for example, by calling a *star* for a *square*). He responded to the female target symbol by calling the male symbol. It is also interesting to note that *waves*, which the subject claimed had some erotic significance, seems to be positively associated with the erotic symbols.

In view of the subject's avowed interest in the erotic symbols, these associations are very meaningful and may give us a clue regarding the seeming rarity of consistent missing. Most quantitative card tests involve the use of symbols which have been designed expressly so that there will be a minimum of similarity between the various targets. In addition, the target material is usually of a nature to excite few if any associations involving deep psychological drives such as the sexual one in the case here reported. These factors may account for the relative infrequency of consistent missing.

If the ESP subject classified the targets according to attributes which each has in common—in the case presented here the categories might be sexual *vs.* neutral symbols—then a successful determination of the correct category of the particular target might lead to consistent missing. This could occur if the subject regarded his task as essentially one of determining only in which category the target belonged and indicated it by calling a symbol appropriate for that category; for example, by calling any neutral symbol when a neutral symbol was the target.

How the targets might be classified would most likely be determined by deeper psychological mechanisms peculiar to the subject rather than by a superficial similarity of one symbol for the other, such as *circle* and *square*. In other words, the meaningfulness of the target symbols might determine whether different targets would be classified together. This suggests that target material which is likely to arouse some deep psychological drives would be more likely to be associated with consistent missing. More complex targets such as pictures might provide such meaningful material. Warcollier cites several cases in his book *Mind to Mind* (**4**, pp. 74-79) which seem to belong to a type of consistent missing in that the subject drew the same (incorrect) response to similar target pic-

tures. Now, we have very little evidence in which deep emotional involvement with psi targets has resulted in consistent missing, principally because such situations rarely exist except as spontaneous cases; and since repetition of a spontaneous experience in the same individual is even rarer, the opportunity for consistent missing to be demonstrated is almost nil.

However, it should certainly be possible to arrange a laboratory situation more conducive to consistent missing without the subject's conscious awareness that such an effect is being sought. One such approach would be along the line reported here by Fisk: the selection of an outstanding subject who has meaningful and deeply emotional associations to certain target cards but not to others. The value of such experiments would lie not in their confirmation of consistent missing as a valid ESP effect, but rather in furnishing clues as to the modus operandi of ESP. For example, one of the current hypotheses of ESP is that it operates by eliminating certain target possibilities from consideration, thereby increasing the probability of a hit. A subject calling standard ESP cards might be able to determine that the target is one of three symbols so that the chance of a hit is increased from ⅕ to ⅓. It this were the case and the subject had a strong associative link between the various test symbols, then consistent missing should be demonstrable. On the other hand, if ESP is able to indicate the correct target card by eliminating all the other target possibilities, this can be considered equivalent to increasing the probability of a hit from ⅕ to 1, and then no consistent missing would be expected. While consistent missing itself would not prove either of these two hypotheses, it would give considerable supporting evidence for one or the other. It would certainly be important to have an answer to this particular question.

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To the Editors:

My review[1] of the *Ciba Foundation Symposium on Extrasensory Perception* contained the following sentence referring to the paper "Some Difficulties in the Way of Scientific Recognition of Extrasensory Perception" by J. Fraser Nicol: "For example, his claim that in the work of Hilton and Baer, control throws gave almost the same average as the experimental throws, seems to have been arrived at by eliminating from consideration the experimental throws with small and large dice (the major part of the total)." The implications contained in this sentence were erroneous and I should like to take this opportunity to correct them. As the control throws were done entirely with medium dice, the only comparison that can legitimately be made is with the experimental throws with medium dice. However, even if the experimental throws with small and large dice had been included in the comparison, the difference between experimental and control throws would not have been significant (contrary to the implication of the quoted sentence). Moreover, the experimental throws with small and large dice were somewhat less than half of all experimental throws reported and therefore were not correctly described as "the major part of the total."

Without wishing in any way to minimize the inaccurate implications of the sentence quoted, I should like to mention in passing that an impartial appraisal of the Hilton-Baer work would have noted the existence of one highly negative scoring subject in the "miscellaneous series." The difference between the scores of this subject W. S. and of the highest scoring subject G. B. is highly significant ($X^2=12.57$, $P=.00039$) and after multiplying by 10 to

[1] *Journal of Parapsychology*, Sept., 1956, pp. 197-203.

allow for selecting one out of 10 possible comparisons, we would still have 10 P=.0039.

The sentence just preceding the one already quoted read: "Nicol's claim of inconsistencies between statements made in popular writings and the research reports on which they are presumably based seems to the reviewer to rest on a biased interpretation of the sources quoted." On further consideration, I now feel that this was an overstatement. What I meant to say was that Nicol emphasizes those aspects of the examples cited by him which would seem to support his thesis (that Rhine's popular writings abound in "rash pronouncements" and "unsubstantiated claims") and does not mention other aspects of the same reports which would tend to weaken his argument. For example, his remarks concerning the work of Hilton and Baer, while factually correct, are calculated to leave the reader with the impression that the claim of "significantly high scoring" was invented for the purposes of the popular book; and one would never guess that Rhine was in effect merely quoting from the original report.

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GLOSSARY

In order to avoid constant redefining of commonly recurring terms in papers appearing in this JOURNAL, the following definitions are submitted for convenient reference.

A simple description of the basic experimental methods and of the statistical procedures to be followed in evaluating ESP (both card tests and drawings tests) and PK may be found in the *Handbook of Tests in Parapsychology* by Betty M. Humphrey (published by the Parapsychology Laboratory, Duke University, Durham, N. C.).

AGENT: In tests for telepathy, the person whose mental states are to be apprehended by the percipient. In GESP tests, the person who looks at the stimulus object.

AVERAGE SCORE: Average number of hits per run.

BT ("Broken Technique"): The clairvoyance technique in which each card is laid aside by the experimenter as it is called by the subject. The check-up is made at the end of the run.

CALL: The ESP symbol selected by the subject in trying to identify a target.

CHANCE: The complex of undefined causal factors irrelevant to the purpose at hand.

CHANCE EXPECTATION = MEAN CHANCE EXPECTATION: The most likely score if only chance obtains.

CHANCE AVERAGE: Mean chance expectation expressed as an expected score, generally in terms of average per run.

CHI SQUARE: A sum of quantities each of which is a deviation squared divided by an expected value. Also a sum of the squares of CR's.

CLAIRVOYANCE: Extrasensory perception of objective events as distinguished from telepathic perception of the mental state of another person.

CR (CRITICAL RATIO): A measure to determine whether or not the observed deviation is significantly greater than the expected random fluctuation about the average. The CR is obtained by dividing the observed deviation by the standard deviation. (The probability of a given CR may be obtained by consulting tables of the probability integral, such as Pearson's.)

CR OF THE DIFFERENCE: The observed difference between the average scores of two samples of data divided by the standard deviation of the difference.

DECK: Twenty-five ESP cards.

CLOSED DECK: An ESP deck composed of five each of the five symbols.

OPEN DECK: An ESP deck made up of the ESP symbols selected in random order.

DEVIATION: The amount an observed number of hits or an average score varies from the mean chance expectation or chance average. A deviation may be total (for a series of runs) or average (per run).

DIE THROW: The elementary unit of data in a dice-throwing test represented by the throwing and reading of a single die, no matter how many dice are thrown at a time.

DISPLACEMENT: ESP responses to targets other than those for which the calls were intended.

BACKWARD DISPLACEMENT: ESP responses to targets preceding the assigned targets (the ones for which they were intended). Displacement to the targets

one, two, three, etc. places preceding the assigned target are designated as (−1), (−2), (−3), etc.

FORWARD DISPLACEMENT: ESP responses to targets coming later than the assigned targets. Displacement to the targets one, two, three, etc. places after the assigned target are designated as (+1), (+2), (+3), etc.

DT (Down Through): The clairvoyance technique in which the cards are called down through the deck before any are removed or checked.

EMPIRICAL CONTROL: An experiment which wholly or partially follows the main experiment with the exception that the conditions are designed to exclude the possibilty of the hypothesis being tested.

ESP (EXTRASENSORY PERCEPTION): Response to an external event not presented to any known sense.

ESP CARDS: Cards, each bearing one of the following five symbols: star, circle, three parallel wavy lines (called "waves"), square, plus.

ESP SYMBOLS: See plate opposite page 1, this JOURNAL, Vol. 1, March, 1937.

EXPECTATION; see CHANCE.

EXTRACHANCE: Not due to chance alone.

FREE MATERIAL: The stimulus objects of experiments in which an unlimited or unspecified range of stimulus objects is employed (as contrasted with methods such as card-calling in which the subject knows that the stimulus object is one of a known range).

FREE RESPONSE METHOD: An ESP test method in which a relatively unlimited range of stimulus objects is used and in which the subject is instructed to express freely as his response any range of thoughts and ideas.

GESP (GENERAL EXTRASENSORY PERCEPTION): A technique designed to test the occurrence of extrasensory perception, permitting either telepathy or clairvoyance or both to operate.

HIGH-DICE TEST: A PK technique in which the aim of the subject is to try to influence a pair of dice to fall with the two upper faces totaling eight or more.

LOW-DICE TEST: A PK technique in which the aim of the subject is to try to influence a pair of dice to fall with the two upper faces totaling six or less.

MEAN CHANCE EXPECTATION; see CHANCE.

P (PROBABILITY): A mathematical estimate of the expected relative frequency of a given event if chance alone were operative.

PARAPSYCHOLOGY: A division of psychology dealing with those psychical effects which appear not to fall within the scope of what is at present recognized law.

PERCIPIENT: The person who makes the calls in an ESP test.

PK (PSYCHOKINESIS): The direct influence exerted on a physical system by a subject without any known intermediate physical energy or instrumentation.

PRECOGNITION: Cognition of a future event which could not be known through rational inference.

PREFERENTIAL MATCHING: A method of scoring free responses. A judge ranks the stimulus objects (usually in sets of four) with respect to their similarity to, or association with, each response; and/or he ranks the responses with respect to their similarity to, or association with, each stimulus object.

PSI: A general term to identify personal factors or processes in nature which transcend accepted laws. It approximates the popular use of the word "psychic" and the technical one, "parapsychical."

PSI PHENOMENA: Occurrences which result from the operation of psi. They include the phenomena of both ESP (including precognition) and PK.

QD (QUARTER DISTRIBUTION): The distribution of hits in the record page (or in a logical subdivision thereof, such as the set or the half-set) as found in the four equal quarters formed by dividing the selected unit horizontally and vertically.

RUN: A group of trials, usually the successive calling of a deck of 25 ESP cards or symbols. In PK tests, 24 single die throws regardless of the number of dice thrown at the same time.

SCORE: The number of hits made in one run.

TOTAL SCORE: Total of scores made in a given number of runs.

AVERAGE SCORE: Total score divided by number of runs.

SD (STANDARD DEVIATION): Usually the theoretical root mean square of the deviations. It is obtained from the formula $\sqrt{npq}$, in which n is the number of single trials, p the probability of success per trial, and q the probability of failure. (For ESP cards, SD $= 2\sqrt{\text{no. of runs.}}$) Sometimes the SD used is that estimated from the observed variability in the scores.

SD OF THE DIFFERENCE: For both ESP cards and PK tests using dice, the SD of the difference is equal to $\sigma_s \sqrt{1/R_1 + 1/R_2}$ where σ is the SD of a single run and R_1 and R_2 are the number of the runs in the respective samples compared. This gives the SD of the difference for run score averages.

SERIES: Several runs or experimental sessions that are grouped in accordance with a stated principle.

SESSION: A unit of an ESP or PK experiment comprising all the trials of one test occasion.

SET: A subdivision of the record page serving as a scoring unit for a consecutive group of trials, usually for the same target.

SEVENS TEST: A PK technique in which the aim of the subject is to try to influence a pair of dice to fall with the two upper faces totaling seven.

SIGNIFICANCE: A numerical result is significant when it equals or surpasses some criterion of degree of chance improbability. The criterion commonly used in this JOURNAL is a probability value of .02 or less, or a deviation in either direction such that the CR is 2.33 or greater.

SINGLES TEST: A PK technique in which the aim of the subject is to try to influence dice to fall with a specified face up.

STIMULUS OBJECT: The ESP card or drawing or other object, some identifying characteristic of which is to be apprehended by the percipient.

SUBJECT: The person who is experimented upon. In ESP tests, most commonly the percipient (though also the agent in GESP and telepathy). In PK tests, any individual whose task it is to influence the objects thrown.

TARGET: In ESP tests, the stimulus object; or in telepathy, the mental state of the agent. In PK tests, the faces of the die (or combination of faces) which the subject attempts to bring up in the act of throwing.

TARGET CARD: The card which the percipient is attempting to perceive (i.e., to identify or otherwise indicate a knowledge of).

TARGET DECK: The deck of cards the order of which the subject is attempting to identify.

TARGET FACE: The face on the die which the subject tries to turn up as a consequence of direct mental action.

TELEPATHY: Extrasensory perception of the mental activities of another person. It does not include the clairvoyant perception of objective events.

TRIAL: In ESP tests, a single attempt to identify a stimulus object. In PK tests a single throw of the dice or other objects thrown.

HERE AND THERE IN THE LITERATURE

Science has lost the public mind in a maze of facts and, with them, their inner meaning. Materialistic naturalism is currently prevailing among many of our people as a result of science. This results in a much stronger case for communism than for our free way of life. If man is created entirely materially, then freedom of will has no meaning. If man is wholly a product of natural law, then individual freedom is surely an illusion and the philosophy of communism is a much better solution for this problem than a system that urges a freedom which cannot, and should not, exist.

I wish I knew the solution to this vexing problem of the impact of a materialistic science on the peoples of the world. We can all agree as scientists that it is our prerogative to go as far as is possible with whatever scientific device we know. That prerogative we must never surrender. As scientists, our philosophy of life is no better than that of other thoughtful people, and in some cases it is a good deal worse because of our lack of experience in the much more populous world of intuition and value judgment.

IRVINE H. PAGE, "Chemistry of the Brain"
Science, April 19, 1957